

# Psychological Review

EDITED BY

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# THE PSYCHOLOGICAL REVIEW

## 'EXPLANATORY PRINCIPLES' IN PSYCHOLOGY<sup>1</sup>

BY FRANK A. GELDARD

*University of Virginia*

The general question I desire to discuss is that concerning the nature of explanation in psychology. What are the consequences of adopting one or another procedure in effecting explanations of our phenomena? We should probably all agree, at the outset, that, once psychology's position in the scheme of things has been settled, once a decision has been reached as to its essential contents, its primary task is that of description. This has been said in a variety of ways and much internal disagreement in psychological theory results from the difficulty, perhaps the impossibility, of reaching substantial unanimity as to what our descriptive terms and categories are to be. In this regard psychology's problem is in no wise unique. All other sciences are currently confronted and, perhaps since the very first frontal lobes discharged in the interest of science, have been confronted with the necessity of exercising selection both with respect to what particular aspects of man's experience are to be set aside for systematic study and what symbols are to be invented for intelligible record of observations.

There are those, and before I am through I shall claim membership among them, who would insist that when description has become complete, science's task is ended. The formula seems over simple and I hasten to add that I believe the descriptive process is never complete and, accordingly, that there is no finality in science.

<sup>1</sup> Presidential address before the Southern Society for Philosophy and Psychology, Durham, North Carolina, April 7, 1939. Certain passages, including all introductory remarks, having interest only in the setting of their original delivery, have been omitted.

In contrast with this relatively straightforward view of science's task is another whose adherents, while allowing, of course, that descriptive facts are the building stones of which the scientific edifice is constructed, hold that a science is much more than an assemblage of facts. It is a unified structure calling for organizational design, rules for relating the facts, and particularly demanding explanatory principles.

Now, one may agree that this latter view is also correct. Nor does acceptance of both positions force one either into neuroticism or conceptual immobility. The relations between facts are of a piece with the facts themselves. One has to assume some kind of intellectual setting before facts appear. A fact is itself a relationship between terms and the experimental method of obtaining facts, so much favored by contemporary science, is but a formalization of some fairly universally accepted rules for effecting the union. Measurement, too, reduces to the same. Look to any acceptable scientific fact and it becomes apparent that the scientific thinker must be exclusively concerned with relationships, else doom himself to ineffability. Unification as a necessary characteristic of science is thus guaranteed at the outset. A scientific description is a great deal more than undirected verbalization. It always presupposes an attitude towards the object of description and, particularly, a getting within some acceptable 'rules of the game,' a postulational system, if you like.

We sometimes carelessly think of scientific description as simply yielding a faithful picture of the world as it is given to us; a favorite figure is "Science mirrors reality." Without entering extensively into the old epistemological argument between the *apriorists* and the *empiricists* as to what constitutes reality—a problem no nearer solution in the present day than it was when first the quarrel broke out—it becomes highly pertinent that we attempt to arrive at some conception of science's descriptive task. Certain it is that the body of science is in no sense a photographic image of some reality external to itself. The interpreters of science, such as Karl Pearson, have drawn attention to the role of sense impressions and have demonstrated the limitations of scientific observation

to be those of illusion and error. A large part of classical experimental psychology and psychophysics is, of course, concerned with these very limitations. But the lack of correspondence goes further, for science is clearly not a description of what is accidentally presented to our senses. The world is given to us, providing we do nothing to order it, in a series of disconnected fragments. Yet we neither think nor talk about it that way. J. S. Mill, in the *Logic* (9, p. 216), has said, "The order of nature, as perceived at first glance, presents at every instant a chaos followed by another chaos. We must decompose each chaos into single facts. We must learn to see in the chaotic antecedent a multitude of distinct antecedents, in the chaotic consequent a multitude of distinct consequents. This, supposing it done, will not of itself tell us on which of the antecedents each consequent is invariably attendant. To determine that point, we must endeavor to effect a separation of the facts from one another, not in our minds only, but in nature. The mental analysis, however, must take place first, and everyone knows that in the mode of performing it, one intellect differs immensely from another."<sup>2</sup> In a memorable passage James makes the point vividly (7, p. 635): "The world's contents are given to each of us in an order so foreign to our subjective interests that we can hardly by an effort of the imagination picture to ourselves what it is like. We have to break that order altogether, and by picking out from it the items that concern us, and connecting them with others far away, which we say 'belong' with them, we are able to make out definite threads of sequence and tendency, to foresee particular liabilities and get ready for them, to enjoy simplicity and harmony in the place of what was chaos. . . . We have no organ or faculty to appreciate the simply given order. . . . As I said, we break it: we break it into histories, and we break it into arts, and we break it into sciences; and then we begin to feel at home. We make ten thousand separate serial orders of it. On any one of these, we may react as if the rest did not exist. We discover among its parts and relations that which were never given to sense at all—mathematical relations, tan-

<sup>2</sup> Mill's catalogue of the differences, later in the same passage, is very discerning.

gents, squares, and roots and logarithmic functions—and out of an infinite number of these we call certain ones essential and lawgiving, and ignore the rest."

Just how is the 'breaking' process that James refers to accomplished? What determines how the sciences are to be arranged? We shall have to confront these questions in seeking an answer to our general problem of the nature of explanation, particularly if we are to see what psychological explanations are. But, for the present, it should have become apparent that the alleged addition of an organizational process to the descriptive one involves an erroneous view of scientific procedure. Science is apparently achieved not so much by adding an ideational superstructure to its descriptive material as by the adoption, in the first place, of a systematic approach of one or another degree of efficacy to bring the descriptions into being.

But what of so-called 'explanatory principles'? Surely they are conceptually closer to the angels! My answer would be that they are just as close—and just as remote—as are descriptive statements themselves. The more one inquires into the nature of 'explanations' the more one finds them to consist simply of further descriptions. To be sure, the descriptions offered in explanation of a particular phenomenon are not haphazardly selected. On the contrary, the aptness of an explanation depends directly upon its intimacy of relation with the fact being explained and one is again forced back upon some conceptual system or set of rules to decide what constitutes intimacy or remoteness of relation.

But perhaps it is not entirely obvious that explanation is really no more than further description. I know of no way of convincing oneself that this is the case short of making an analysis of the explaining behavior of the scientist and through a multiplication of instances inducing the rule. But let us consider a relatively uncomplicated illustrative case with the hope that at least the pattern may be suggested.

I observe, in looking at a certain beetle (*Plusiotis resplendens*), that its apparent color is approximately that of dull brass, but that on viewing it from another angle it seems quite

reddish. (Note the relations involved in obtaining the fact, including the use of the experimental method at a primitive level.) I go to an expert in these matters in search of an explanation of the color change. He tells me that many beetles, moths and butterflies, as well as hummingbirds, peacocks and certain pigeons, possess these beautiful colors, the chief features of which are an extremely high reflectance value, especially for some particular color viewed at normal incidence and a shift of color with change of angle towards the shorter waves, though there seem to be some exceptions to the latter rule. These extraordinary colors, he says, are known as metallic colors and they occur almost not at all in the vegetable kingdom. Being a dilettante in such matters I am likely to say, "Oh, I see," and depart somewhat smug in my new knowledge. An explanation of a sort has been received. But if I am somewhat more persistent—and I hope I should be less moronic than this hypothetical account would make me—I might reasonably ask to be told more about the nature of systematic shifts in metallic colors, particularly since the change I observed was in the unusual direction of the longer waves. Thus far I have only learned that they occur more commonly than I had supposed. My expert goes on to tell me about selective absorption of light waves, interference phenomena, elliptical polarization of light by metals and the optical properties of thin layers of the aniline dyes. Depending almost entirely upon my degree of sophistication in these matters, but somewhat on his patience, to be sure, he may find it necessary to recount the essential facts concerning channeled spectra, certain characteristics of potassium chlorate crystals, the photochromatic interval and Purkinje shift, Michelson's brilliant comparisons of the optical behavior of feathers, butterfly scales, and beetle wing-cases with those of dye films of a thickness of one-tenth of a light wave (8, Chap. 15). All these things, indeed, are definitely a part of the story. At some point I shall either say, "Oh, I see," or decide that the explanation is incomplete (which, in point of fact, it is and always will be). You will have observed that "Oh, I see," or its symbolic equivalent, marks the terminus of every

attempt at explanation. It is a signpost on the endless road to understanding. There are times when I think that pedagogy consists of nothing but a progression from one "Oh, I see" to another. Certainly the reaction is highly symptomatic and if one ever finds, in academic teaching, this response to have become extinguished, it is time to begin worrying about salary checks, if not about one's own soul.

An explanation, in the case of our beetle, might have been arrived at somewhat more simply had the explaineer entertained a different set of notions as to what constituted acceptable scientific facts or had set up a conceptual system in which less commonly approved relations were admitted. Thus the explanation in this case might have been that beetles, spontaneously and without warning, sometimes get attacks of erythema. Or, if prior knowledge would permit it to be credited, it might have been that pixies are accustomed to dragging colored veils over beetles who are stared at too intently. A second feature of explanation, its personal character, thus suggests itself. An explanation is a further description *to* someone and whether or not it will be satisfying to a particular person will depend to a considerable extent on his metaphysical presuppositions which, in turn, are apt to be accidents of training and familiarity with various scientific contents.

Furthermore, what constitutes acceptability of explanation is determined to a great extent by the general scientific setting of the time and place. Three hundred years ago, immersed in the science of his day, one would have done well to have accepted, for example, the explanation that visual after-images result from the outward projection of light previously absorbed by the eye. Such, indeed, was the explanation offered by the Jesuit, Kircher, in 1646, and presumably there was available no account more harmonious with the visual science of the time (5, p. 261).

While, then, the basic procedural pattern in explaining seems to be simple, the specific forms it may take are obviously endless in variety. To the systematic scientist all must be explained in terms consistent with his prior knowledge or, at

least, in conformity with a logical regimen he has made his own. I should be the last to decry such orderliness, but there are definite dangers in preoccupation with a single point of view. Huang (6), in his study of children's attempts at explaining strange phenomena, seems to have found operating in rather clear fashion some of the factors barring successful problem solution. It appears that, at least with children, existing knowledge hinders the application of new thought to a problem and at the same time does much to insure that only selected aspects of the problem are brought into agreement with the existing knowledge. These are childish habits of thought not commonly put away by the average man of science. Too great preoccupation with a systematic viewpoint can even become obsessive. Haldane (4, p. 9) reports that, as a student, after having attended a few post-mortems, he came to realize that "even the ugliest human exteriors may contain the most beautiful viscera" and used to console himself for the facial drabness of his neighbors on omnibuses by dissecting them in his imagination.

A colleague was recently prey to the same general sort of selective influence. He was recounting to his class the essentials of Watson's now classic experiment with little Albert, the loud sound and the white rat. Having told clearly how the fear response to the sound of the steel bar eventually became attached, through conditioning, to the sight of the rat, he paused for possible questions. Only one came, and it was disconcerting. It was, "What happened to the rat?" Watson doesn't say, and he is known to have been excessively concerned with rats.

Of course, this danger of narrow thinking exists only for the systematic scientist. There are a great many conceptual nomads in scientific circles who, far from demanding that descriptions offered in explanation be ordered or consistent with a point of view, will accept as explanatory any further description, provided only that it be interesting. Intimacy of relation is for them 'all-or-none,' not to be charted on a quantitative scale. However unstabilizing for science, this attitude is at least refreshing. I am inclined to think that the

body scientific, like the bodies politic, economic and social, is improved by the leavening influence of iconoclasm.

Perhaps I am only saying that there are many persons disporting themselves in scientific company whose interests are essentially technological, for it is precisely lack of systematic adherence to a single ideational framework that marks the thought of the technologist. He is under no other compulsion than to cling with steadfast purpose to a practical aim. He may appropriate knowledge wherever he finds it; he may, indeed, work continually in a conceptual twilight, provided only that his practical ends are realized. The results of such efforts are likely to appear very curious when examined from within scientific prejudices. I have in my possession a very respectable book called *Illuminating Engineering* (2), a fair sample of one such result. It deals among other things with dining-room illumination, quantum theory, railway headlights, pathological conditions of the eyeball, the virtues of gloss finish paints, the moods induced by colored lights, department store show windows, fireflies and their habits, lenses for lighthouses, the microscopic structure of the retina, and the principle of the boulevard stop! And it is a good book; its subject matter is organized in relation to a very real purpose, not by a systematic logical position with respect to the universe.

Now, what are the consequences for psychology of the general view of the nature of explanation already expressed? Among what relationships does psychology find its 'explanatory principles'? What kind of further description is admissible and pertinent? Some decision as to the position of psychology among the sciences would seem to be involved. A useful figure was once suggested by Professor Titchener (11). He pictured the "world of experience as contained in a great circle, and . . . scientific men as viewing this world from various stations upon the periphery. There are, then, . . . as many possible sciences as there are distinguishable points of view about the circle. . . . no one of them in truth exhausts experience or completely describes the common subject-matter, though each one, if ideally complete, would exhaust some as-

pect of experience." This spatial analogy seems to me to be helpful because it draws attention not only to a broad view of the metaphysical problem of how science as a whole is constituted, but also to the intimacy of relation between the sciences, represented in the peripheral dimension. The forces holding sciences together are far more potent than the disrupting ones pulling them apart: the history of science shows it to have become less departmentalized as it has advanced. At present all attempts to classify the sciences by a partitioning technique seem mildly amusing. No one doubts that in a general sort of way one can distinguish between them, somewhat on the basis of content and to some extent on the grounds of their use of the more sophisticated forms of measurement. In the middle of the last century a fairly lively controversy ensued between the followers of Comte and those of Spencer as to which were concrete and which abstract sciences. There are many artificial ways of classifying the sciences and metaphysics is the gainer for the attempts at classification having been made. Things in our surroundings do seem to fall into groups and unless we are alert we may think the falling to have been spontaneous and necessary. Few would hesitate to say, offhand, that the incline plane belongs to physics, yellow fever to pathology, and recorders of emotional expression to psychology. Yet recent literature reveals the incline plane to take on special significance in the investigation of the geotropic behavior of the white rat, the isolation of the yellow fever virus to have been a by-product of efforts to produce high rotational speeds in the laboratory of pure physics, and improvements in the plethysmograph to have come from pharmacology. Nor is this a temporary transposition of interests. It is conceivable to me that ecology or astrophysics, say, might be interested in all three.

Just how uncompromisingly, then, is the individual scientist to be expected to stick to his position on the circle? How far may he be permitted to wander from his station and look at experience from other points of view? My good friend and colleague, Professor Balz, in his presidential address before this Society (1), chided psychologists for their "metaphysical

infidelities" and their proneness to take all knowledge as their province. I believe a real point to have been made. But so long as one keeps some proper notions of relativity and retains his orientation sufficiently to permit his recognition of someone else's observation platform when he treads on it, there is nothing unhealthy either for science at large or for particular sciences in these journeyings. I should in the same breath caution that the Cook's tour of experience may be in bad taste and that too great a peripheral velocity may lead to what Professor Boring has called 'epistemological vertigo.' There are undeniable tangential dangers in such an exercise, but they seem to me to be no more hazardous than is strict preoccupation with a sheltered view of the world. It is possible for the thinking man to find science, all science, congenial and to benefit enormously by permitting himself countless vistas of reality. For the psychologist, dealing as he does with an order of complexity of content not met with in any other of the natural sciences, such a devotion to science at large is not only advisable; it is mandatory if he wishes to avoid a 'flight from reality.'

In devising explanations the psychologist will, of course, wish to concern himself first of all with obviously related facts discoverable within his own observational system. The breadth of his explanatory activities thus derives from the dimensions of his system. If there are to be discovered any sharp contrasts in current psychological theorizing it is that between the strict system-builders, who confine themselves to a narrow range of terms and a meagre set of postulates, and those whose interests and efforts know no such limitations. There is undeniable satisfaction in getting within a tidy descriptive scheme, where all terms permit of but a single meaning and all systematic relations are known in advance. Results are assured from the start and one knows at the end a great deal more of what he knew at the beginning. That is advance of a kind. The adoption of a closed system and rigid adherence to inflexible rules of observation make for the same sort of satisfaction as that accompanying the performance of any other relatively perfected skill. Swimming, teaching sta-

tistical method, or participating in the modern dance all derive from similar motives. They yield the sheer joy of unimpeded activity. This is, of course, the original 'pleasure principle,' doubtless known to the first polar bear sliding down his cake of ice. No, there is no vexing explanatory problem when one has at hand all the explanation he needs at the outset.

But many relations discoverable outside one's own observational system are quite as urgent. Many psychological facts lead quite naturally to explanation in physical, chemical, anatomical, or physiological terms. The one relation of greatest immediacy for psychology is that with nerve physiology. If there is one generalization that can be counted on to hold good when other, less embracing ones, have fallen into decay, it is the classic "*Nemo psychologus nisi physiologus*." An earlier generation of psychologists would hardly have needed to be reminded that a fundamental descriptive relation exists between psychology and nerve physiology. Indeed, so intricately were neural principles woven into the fabric of psychological theory that, in retrospect, the quarrels between the 'schools' are all but unintelligible apart from a consideration of their ways of dealing with the nervous system. An attempt, for example, to show systematic differences between the later existential psychology and Gestalt theory would be futile without recourse to 'brain fields' and 'dynamic equilibria.' Had the reflex arc, with its implications for the 'long section' view, never been conceived, American functionalism and its product of youthful folly, behaviorism, might never have appeared upon the scene. Neurological fact has always been an insistent explanatory vehicle for modern psychology.

As in former times, the current scene in psychology lends itself readily to analysis in terms of relations with neural fact. Not more than a score of years ago it might have been said with all propriety that psychological fact was far in advance of relevant neurological fact. There was at hand such a bewildering array of phenomena requiring explanation that it appeared that psychologists would either have to mark time until some neurological discoveries were made or become neu-

rologists pro tem. The prediction could not then have been made that many psychologists would actually adopt the latter course and that within a brief span of years the facts of nerve physiology would have become amassed to such a point as to have sent psychologists scurrying back to their laboratories in search of phenomenal facts, explanations for which were already available. The program of dissecting, narcotizing, shredding, and otherwise assaulting the nervous system has been as much a part of the story of modern psychology as of neurology.

Perhaps as cogent a way as any of classifying and evaluating our contemporary problems is with respect to their intimacy with the known facts of neural structure and function. When I speak of 'known facts' I am thinking, of course, of the experimental revelations within neural anatomy and physiology that have led to persuasion on the part of the scientific sophisticate. There is no other acceptable criterion of 'correctness' of facts. I am *not* thinking of the dream world, populated by ever-deepening neural grooves to account for learning, wandering dendrites invoked to care for the various phenomena of attention and sleep, central switchboards through whose magic operation variability of behavior becomes a problem solved for all time, 'engines' of the brain responsible for the abilities of man, or other fantastic inventions of psychologists who feel themselves under the compulsion to explain before they are quite sure what has to be explained. This is clearly not the place to attempt a catalogue of psychological problems, arranged in accordance with their relevance to neural fact, but it would be an amusing exercise. Of our two largest experimental fields, sensory processes and learning, the former would come off rather well at present, I think, while the latter would be found to be in something of a plight.

It is notable that those who spurn neural principles of explanation are mainly those who are primarily interested in the learning process. There is reason enough for them to regard neural explanations as somewhat inexpedient. They are chiefly concerned, in their experiments, with the manipula-

tion of environmental conditions and the observation or measurement of some selected aspect of gross behavior. The stimulus-response formula for them involves large segments of behavior; the relations between the end-terms of the formula are remote ones. But if the stimulus ever gets defined as it ultimately will have to be, in terms of the organism instead of in terms of the external environment, or if workers in this field were to become more explicit as to what they mean by 'response'—the 'envelope'<sup>8</sup> of tonic contraction, *e.g.*, instead of "the subject makes an error" or "the animal approaches the goal"—the neural events involved would suddenly come to be of the first order of importance to them. As it is, they cannot afford to neglect neural guesses about learning. Lapicque came close to providing a passably good explanation of many forms of learning with his principle of "chronaxic switching" of a few years ago. The principle has, to be sure, succeeded in getting itself switched off on a sidetrack, though perhaps only temporarily, through difficulties internal to neurology. And there have been other hypotheses that have 'sounded' essentially right. One cannot afford to pass by these attempts at supplying master neurological principles. It is true, of course, that imagination appears to have been particularly fertile where speculation as to the neural basis of learning is involved. Some extraordinarily bizarre notions have found their way into the public prints. Documentation of that statement would even require unearthing some minor intellectual scandals. It would be unwise to advocate at the present time that neural hypothesis be elevated to the commanding position in learning research. Nor does there appear to be any advantage in redefining learning problems in terms of hypothetical nervous processes, as Cason (3), *e.g.*, has done. One then plunges into a world of fantasy—and that way lies madness. But, all in all, one would think, from the rejection of neural principles by learning theorists—*e.g.*, witness Skinner's recent book (10)—that the nervous system has only mythical existence!

I have perhaps laid disproportionate stress on related

<sup>8</sup> In a meaning such as that used by sound engineers.

neural description as a 'principle of explanation' for psychology. The ready applicability and obvious intimacy of such descriptions force one rather naturally in such a direction. Neural fact does not, of course, constitute *the* principle of explanation. There are other observation platforms in which the planking has been cut from the same forest as that supplying psychology's footing. Psychological explanations have come from many related disciplines and the indications are that more, not fewer, relations will be invented. And within the field itself an ever-increasing terminology and set of significances broaden the explanatory base. Instinct, vector, resonance, purpose, reintegration, insight, regressive libido—all these terms, and a thousand more, are names of 'explanatory principles' to the extent that they are genuinely descriptive of something. To the extent that they are descriptively meaningless they are just words and there is no reason whatever for us to put up with their tyranny. To explain is perforce to describe, and to describe within a field of relations or with respect to a frame of reference is necessarily to explain.

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## TOWARD A THEORY OF CONDITIONING

BY ORVIS C. IRWIN

*Iowa Child Welfare Research Station  
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The manner in which the title of this paper is worded—*toward* a theory—implies a conviction that aside from Pavlov there exists no fully-developed systematic theory of conditioning. There are, however, some experimental data which may be used for the beginnings of a new formulation. A great deal of incidental questioning, if not outright criticism, is current in the literature on the subject, although it has not resulted in a concerted attempt to restate the conventional viewpoint. This situation is probably due to the enormous prestige of Pavlov's name. One should not be unmindful, of course, of the possibility that whoever proposes to raise questions concerning the underlying assumptions of a great scientific enterprise is himself vulnerable. On the other hand, he may be fortified by the knowledge that in the free atmosphere of modern science there are no faultless and enduring Messiahs, so that the prestige of Pavlov's achievement need not deter him from entertaining a healthy scientific skepticism of a powerfully entrenched dogma.

After summarizing in a preliminary way the main results of some experiments on the conditioning process in infants, certain items of data which seem to be pertinent to the problems of interpretation will be emphasized. The article is organized into four parts: the first will deal with our own experimental results; the second will outline Pavlov's general position; the third will consist of a critical estimate of his fundamental assumptions; and the last will attempt a different interpretation, especially of the somewhat knotty problem of internal inhibition.<sup>1</sup>

<sup>1</sup> The actual experimental work on which this discussion is based was performed by Drs. M. A. Wenger and Ruth Wildenberg Kantrow and to them goes the credit for originating any positive contributions toward a theory. My part is merely that of a catalyst and precipitating agent.

## THE EXPERIMENTAL RESULTS

Previous to Wenger's (30) study, the only really systematic investigation of conditioning in newborn infants in America was done by Dorothy Postle Marquis (21). Wenger's primary object was to make an exploratory study into the possibility of conditioning a variety of different stimuli. For conditioned stimuli he used a cutaneous vibrator, a tone, a light, and a buzzer. The unconditioned stimuli were a light, a shock, and food (milk). He paired these conditioned and unconditioned stimuli in various combinations. The first experiment involved the pairing of tactile vibrations and light in an attempt to condition eyelid responses to touch. In other experiments he investigated the conditioning to a tactile stimulus of a withdrawal response from electric shock, of a withdrawal response to a sound, and also to light. Infants were used from their second to their eighth day. In all these experiments the criterion of conditioning was a differential score between the responses of experimental and control groups.

Wenger (30) confirmed Marquis' (21) finding that conditioning may be set up during the first week of life, but he demonstrated that it is of an unstable nature. Only six of fourteen infants were conditioned, the remaining cases giving unsatisfactory results. Best results were secured when electric shock was used as the unconditioned stimulus. A very large number of pairings are required to condition newborns, varying from 120 to 280. Wenger (30) demonstrated the presence of a variety of conditioned phenomena such as anticipation, external inhibition, and disinhibition. Having a considerable bearing on the interpretation of the conditioning process is the observation that while the infant is in a state of complete muscular quiescence the vigor of the response decreases and finally drops out.

Kantrow's (14) study was done on sixteen infants between the ages of six weeks and four months. The conditioned stimulus was the sound of a buzzer; the unconditioned stimulus was milk. Kantrow was interested not only in the problem of conditioning the sucking reaction, but also in the much

neglected problem of the effect of conditioning the sucking reaction upon other concomitant behaviors, such as body activity, crying, etc. From the theoretical standpoint, therefore, her study is more broadly based than the conventional conditioning experiment. Analyses of the rates of acquisition and of decrement of the responses were made. In all of her experiments, she adopted a method which insured the presence of controls, thereby obtaining differential scores as indices of conditioning. Another matter of theoretical importance is the fact that the experiment was set up with a view to obtaining data on the effect of the internal state of the infant, such as hunger and satiety.

Fifteen of the sixteen infants were successfully conditioned, a much larger proportion of the group than in the case of Wenger's subjects. Vincent curves were constructed to show the rate of acquisition. These curves reveal an initial acceleration and then flatten out into a plateau. An item of considerable interest is the fact that with continued reinforcement the curve shows a decrement, which in turn achieves a plateau at a lower level than the first plateau, but still significantly higher than the values for the controls. She also showed that during the state of satiation little progress in conditioning occurs.

#### PAVLOV'S WORK AND HIS THEORY OF CONDITIONING

The empirical results of Pavlov's work on conditioning, including those on the generalization and specificity of stimuli, have been amply confirmed in many experiments until they are as certainly established as any datum in modern physiology and psychology. Let us take a look at Pavlov's explanation of them. For Pavlov, as for most neurophysiologists, there are at least two processes in the central nervous system, the processes of excitation and inhibition. To elucidate their actual working, he has recourse to two phenomena which apply to both of them. These are irradiation and concentration of nervous impulses and he is confident that they have been established under firmly fixed quantitative laws. They have spatial and temporal characteristics. When in the ex-

periment the point on the shoulder of the dog is stimulated, a corresponding spatial point in the dog's cortex is excited. In time this point of cortical excitation tends to spread to other spatial points on the brain. Irradiation of cortical excitation, therefore, according to Pavlov, accounts for the phenomenon of generalization. Later the irradiation recedes once more upon the originally excited point. This is concentration and is intended to explain specificity of salivation to the single shoulder point. In the same manner, he explains inhibition. While salivation to the point on the dog's rump is being extinguished by failure to reinforce with food, a nervous process at a corresponding cortical point is inhibited. Inhibition then irradiates to other points, and is followed by a recession or concentration upon the originally inhibited point. This is the explanation offered for his doctrine of internal inhibition, which for him is the crucial concept in his general theory.

These laws were arrived at on the basis of six different investigations on the nature of internal inhibition. To illustrate, let me describe a simplified typical experiment. Suppose we select four equidistant points on the dog's side, one upon the shoulder, one behind the shoulder, and one on the flank and the last one on the rump. Let us condition the first three points equally, so that upon stimulation they each yield, say, ten drops of saliva. Let us, on the other hand, extinguish the point on the rump so that it gives zero drops. We are now ready to perform the experiment crucial to the establishment of his laws of irradiation and concentration. In the experiment, the point on the rump is tested twice in order to verify the presence of extinction. Then at selected intervals the other three points are stimulated, and the resulting drops of saliva are counted in order to determine quantitatively the extent to which the irradiated inhibition from the rump has influenced them. Suppose the flank point now gives only five drops, the side eight, and the shoulder its original number of ten drops. We will then say that inhibition has irradiated to this amount. Continuing the testing we can determine when the shoulder point will yield zero drops. Moreover, we can test when recovery occurs at this point and the time it re-

quires to occur at each of the other two points. The rump point will not show recovery. This reverse process is called concentration. Pavlov's (24) statement of the results of these experiments is included in his address on physiology and psychology, "Our explanation is purely physiological, purely objective, and purely spatial. It is obvious, that in our case the skin is a projection of the brain mass. The different points of the skin are a projection of the points of the brain. When at a certain point of the brain, through the corresponding skin area on the shoulder, I evoke a definite nervous process, then it does not remain there, but makes a considerable excursion. It first irradiates over the brain mass, and then returns, concentrating at its point of origin. Both these movements naturally require time" (p. 273).

#### CRITICAL ESTIMATE

There can be only occasional quarrel with Pavlov's data. This will become apparent as we proceed, but it is not the main issue. However, in my opinion, the assumptions underlying his interpretation of conditioning phenomena need reanalysis.

There are seven such assumptions. If we can raise a reasonable doubt about some or all of them, then presumably the theory will need revision. The direction which this revision should take and the fundamental thesis which I shall try to explicate is that in a typical experiment you do not condition only the single response, you condition an organism. From this thesis there arise several alternative principles to that of Pavlov. The following paragraphs consider Pavlov's assumptions.

1. In his Oxford volume Pavlov (23) devotes a whole chapter to the conditions under which the successful outcome of a conditioning experiment may be achieved. Foremost among these is the necessity for a temporal overlap of the conditioned and unconditioned stimuli. He emphasizes this overlap or contiguity in spite of the fact that it is not necessary in setting up his trace reflex. On the score of contiguity he left himself open and his critics have taken advantage of it.

As a matter of fact, there are situations in which the process of conditioning fails even though contiguity is meticulously observed. If Kantrow did nothing else, she demonstrated that contiguity is not enough. She reports, "The curve for the frequency of sucking drops precipitously during the last part of the experimental feeding period to a point below the level of the critical control period. This striking drop . . . is associated with a complete loss of the conditioned feeding response" (p. 52). In other words, besides the item of contiguity, the state of hunger or satiety of the organism is a decisive factor in the process. Now there are similar items in Pavlov's own data, which should have made him wary in his emphasis upon contiguity. The point here, not at all new by any means, is that conditioning is as much a matter of the organism as of either the stimulus or the response *per se*.

2. A second assumption, which underlies the work and the conceptualization of the whole Russian school, is that the organ of conditioning is the cerebral cortex. In fact, on Krasnogorski's last visit to America, he stated in his lectures that the term 'conditioned reflex' in Russia has been displaced by the term 'cortical reflex.' That the cortex is the organ of conditioning is an original motif of Pavlov's work. Without it, there is no rationale to his experimental efforts. But this proposition no longer is the dogma it once was. The evidence from a number of American laboratories, especially the work of Marquis (20) at Yale and Culler (6) at Illinois, convincingly indicates that subcortical levels of the nervous system may be conditioned. Correspondingly there is a widespread view held by neurophysiologists, that the cortex of the human infant at birth is nonfunctional, that the infant is a thalamic organism. Pavlov and especially Krasnogorski consequently held that newborns could not be conditioned. Dorothy Postle Marquis (21), however, did condition newborn infants. Since she also accepted the nonfunctional view of the cortex, she was driven by the results of her experiment to assume that conditioning in neonates is mediated by subcortical structures. Wenger (30) showed that, although conditioning may be established after much labor, the result is not very stable.

Kantrow's (14) work makes it clear that by the end of the second month conditioning can be satisfactorily achieved. These results may mean that the cortex at birth is incipiently functional if not to a very great degree, and that this function during the first months has developed enormously. On the other hand they may mean that neonatal conditioning is a subcortical function, which by the third month has been taken over by the cortex, captured, so to speak, from lower centers. It is a nice question and difficult to answer. The problem has been introduced here because it raises a question about the validity of one of Pavlov's fundamental assumptions, namely that the cortex is exclusively the organ of conditioning.

3. Pavlov assumed that since conditioned responses are cortical reflexes they may be treated from the point of view of a reflexology. In fact, it was his explicit intention to extend the notion of the spinal reflex to the cortex, he himself considering this to be his greatest contribution to science. In his lecture on the higher nervous system, he said a little grandiloquently, "The time has come, gentlemen, to add something to this old notion of the reflexes, to admit that, parallel with this elementary function of the nervous system to repeat pre-formed reflexes, there exists another elementary function—the formation of new reflexes" (p. 224). He pointed out that there has not been heretofore "a general acceptance and systematic application of this formula in the study of the higher parts of the nervous system." Now the logical and practical implication of the conventional reflex theory is that complex behavior is the outcome of the association or integration of units; this view is very explicit throughout Pavlov's writings. Here it is evident that time has just outrun him. While we must undoubtedly find a place for integration in the course of development, it cannot at present be accepted as an exclusive principle. More and more evidence is accumulating against the view. Reflexes and much specific behavior are products of a process of differentiation rather than a sheer integration of units by a cortical process. One needs to remind himself only of the long series of researches, contemporary with those of Pavlov, by Coghill on the development

of behavior of *Ambystoma*. Similar results to those of Coghill have been found in the behavioral development of the rat fetus by Swensen and by Angulo, on the cat fetus by Coronios, and by Tuge in the reptile and bird. That differentiation as well as integration of reflexes and other specific behavior is a principle operating in the development of the human fetus is suggested by the observations of Minkowski, of Baloffio and Artom, and quite recently has been substantiated in studies by Hooker. All this work quite unsettles the foundation upon which a rigid reflexology rests.

4. An assumption, which is not unequivocally supported by experimental evidence, is Pavlov's doctrine of spatial projection. In the experiments outlined earlier, it will be recalled, four points on the skin were conditioned. Pavlov stated in regard to his results: "Our explanation . . . [is] . . . purely spatial. . . . The different points of the skin are a projection of the points of the brain" (p. 273). This statement was made in an address delivered in 1916. Shortly before Pavlov made this statement T. Graham Brown, without rejecting *in toto* the doctrine of localization, had completed some related investigations on this same question of spatial projection. In essence, his experiments consisted in stimulating a series of cortical motor points at stated intervals and recording the peripheral responses. He then reversed his series; that is to say, he stimulated the series of points in an opposite direction. The second sequence of peripheral responses was not invariably the same as the first. He also varied the experiment by stimulating adjacent points and interpreted the results as indications of instability of cortical points. Had Pavlov known of these results on the instability of the cortex, his concept of spatial projection would not have been formulated quite as rigidly as it was, and his theory of inhibition very likely would have been set up differently.

5. Earlier in this paper, Pavlov's theory of irradiation and concentration of nervous impulses from and upon a cortical point was outlined. These laws are the keystone of the general concept of internal inhibition to which he devotes a large portion of his ablest book, the Oxford volume (23). While

the whole of his theoretical construction stands or falls upon the dubious assumption of rigid spatial projection, there is another demonstration of the inadequacy of these laws. It will be noted in all of Pavlov's theoretical discussions of conditioning that his explanations tacitly assume that only one response, the one in question, is conditioned. The laws of irradiation and concentration are always presented in this guise. This was perfectly natural, for during his professional career Pavlov conditioned few responses other than salivation in the dog. Now when one sets up a conditioning experiment, one selects a response to work with, which is simple, definite, and readily isolated. Then one constructs a recording device which will clearly register that particular response together with its peculiarities. When one has, by dint of whatever inventiveness he may possess, set up the device, one proceeds with the recording of his data. The salivary response is just such a response (and there can be no caviling that in recording it Pavlov meticulously isolated it from other responses). By the very conditions of his experiment, however, one has eliminated the recording of all other responses of the organism. For the purposes of any given experiment, this is a justifiable procedure, for the investigator has the privilege of selecting his own problem. But for the purpose of formulating an adequate theory, it is questionable. For in the experiment, one is conditioning not only the one little response item in which he is interested, but also concomitant or associated responses. In a word, one is conditioning an organism.

When Kantrow conditioned the sucking response in infants, we have seen that other responses were conditioned concomitantly. This makes it probable that whole congeries of responses were set up. What bearing, then, does this fact have upon the laws of irradiation and concentration? Perforce all of these simultaneous conditionings must be explained according to Pavlov's theory in terms of spatial projection and of irradiation and concentration. Inexorably one is driven by the logic of Pavlov's thesis to the view that many irradiations and concentrations are proceeding simultaneously to and fro over the brain mass during an experiment in which

many responses are being conditioned. This places too great a burden on Pavlov's laws, especially on the possibility of a concentration occurring upon a point.

6. Pavlov's account of what happens in the nervous system when an impulse arrives there is hardly accurate. For he has repeated on a physiological level the old *tabula rasa* error of John Locke. Implicated in his view of the nervous system is the assumption that it is an inert physiological substance receiving at definite points certain impulses, which after the lapse of time irradiate to other definite points. The tacit assumption is, that when these points are not activated, the nervous system is a passive structure. The fact is, however, that there is nothing static or passive about it. It is continuously being bombarded by proprioceptive impulses from the muscles, joints and tendons, by interoceptive impulses from the sense organs located in membranes lining the body cavities, and by impulses arising in external peripheral sense organs. The sense organs vary on this central activity; sometimes, as in sleep, they are greatly reduced in volume and intensity; at other times, one or the other varies in the degree to which they influence the central nervous system, and still at other times, they may co-operate in an increased bombardment. At any given moment there probably is an organized pattern of central neural activity, which innervates the response system of the organism. Any single new impulse does not get through this central pattern as over an insulated pipeline to result in a certain definite response of a particular motor organ. The single impulse, conditioned or otherwise, may modify the central pattern, may distort it, facilitate or inhibit parts of it, but the resulting peripheral response is the outcome of this modification in the ongoing neural pattern, and not of the single stimulus directly influencing the particular response by means of a neat and simple scheme of irradiation and concentration. If this situation is true, then the laws of irradiation and concentration together with Pavlov's formulation of internal inhibition based upon them cannot be used as the explanation of the generalization and specificity of conditioned behavior.

There is good experimental evidence against his view. Travis (29) showed that impulses in the Achilles tendon reflex of the rat arrive not only at an appropriate level in the spinal cord, but that they also arrive at several places in the brain. He placed electrodes in several areas and upon stimulating the Achilles tendon, found that impulses were recorded with similar latencies at different points. Similar results have been reported by Leese and Einarson (16) and by Gasser and Graham (8). There are other experiments done by Bishop (1) and his students which further elucidate how impulses arrive at the cortex. If the brain of the cat is exposed, electrodes placed over the auditory and adjacent areas, and the ear stimulated, impulses are found to arrive in the auditory regions, but simultaneously voltages are picked by the electrodes in outlying regions. An interesting observation is that there is a gradation of voltages from a place within the auditory area tapering off to surrounding parts. Superficially this bears a resemblance to Pavlov's irradiation, but the time and spatial factors are handled differently, and the concentration phenomenon is not present in these experiments.

#### TOWARD A POSITIVE VIEW

So much for a critical estimate of Pavlov's assumptions. What can we say on the positive side? What view or views of conditioning are available as a substitute for the concept of irradiation and concentration?

Since the numerous conditioning experiments have yielded much data, it may be economical to select only a few but diverse samples. Moreover, in this way we can learn whether a general theory is at all possible or whether we must be content for the present with several special theories.

Three such diverse samples of data are available in the work with infants, one by Wenger (30), and two by Kantrow (14). Wenger reports that in the midst of a conditioning experiment after a series of successful trials, conditioning lapses and then spontaneously reappears. Kantrow reports that toward the end of a given experimental period when the infant is satiated, conditioning fails utterly. She reports also

a very interesting finding regarding a decrement in the strength of conditioning which occurs with continued reinforcement. From the ninth to the fifteenth training unit the curve of acquisition becomes relatively horizontal. Subsequently, even with continued reinforcement, it descends from this plateau to attain a second plateau at a lower level. The second plateau nevertheless still indicates the presence of the conditioned state. The characteristics of each of these items of data are different. In the first instance apparently the same stimulus which is originally excitatory becomes inhibitory and then again excitatory. In the second, a complete decrement occurs with continued feeding, and in the third a partial decrement is superimposed upon an excitatory condition.

In order to find the best fitting explanation to these three diverse phenomena let us survey the present stock of theories, especially theories of inhibition. Such a list will be found to fall into two groups: (1) ultimate or technical theories, and (2) intermediate theories. The former are concerned with the bioelectrical, mechanical or chemical nature of inhibition. They include: Lucas' refractory phase theory; Lapique's heterochronic view; Howell, Sherrington, and Hoagland's chemical views; Ostwalt and Lillie's membrane theory; McDougall's drainage theory; and Max Meyer's deflection theory.

By intermediate or special theories I mean those which make no assumptions concerning the underlying chemical, mechanical, or bioelectric nature of inhibition, and which make no claims toward all-inclusiveness.

Such a theory of course is Pavlov's notion of irradiation and concentration, as is also the peripheral theory. Wenger has proposed a theory of proprioceptive inhibition which I prefer to designate, for reasons which will appear, as a tonus hypothesis. A final theory belonging to this group is the theory of competing reaction systems which we all use when our favorite hypotheses break down.

Let us begin with the refractory phase theory of Lucas (18) and apply it to the first of our three items of data. This view

states that when the wave of electronegativity sweeps down a peripheral nerve or axone, it leaves behind it an area inexcitable to stimulation. This period of inexcitability, the refractory period, is found to be divided into an absolute and a relative refractory state. Just as the refractory condition sweeps along behind the wave of negativity, it in turn is followed by a supernormal state of excitability. So much for refractory decrement. But now let us look at Wenger's (30) datum. He found that during a conditioning experiment with babies a period of positive conditioning, for no apparent reason, would be followed by a lapse of the response. This of course looks like experimental extinction. But he also reports that a brief period of hypersensitivity ensues to both the conditioned and unconditioned stimuli. You will note that at first glance there seems to be a perfect parallel between this datum and the refractory phase theory. The resemblance however is only superficial for the following reasons: (1) the refractory phase decrement in axones is a matter of thousandths of a second. In Wenger's experiment it is many minutes. Thus the decrement in the conditioning situation endures for periods never contemplated by the theory. (2) Refractory phase phenomena were worked out experimentally upon the peripheral axone; they have not been experimentally established for central nervous tissue. On the contrary there is reason to question whether the electrical pattern in the brain tissue, which consists of a multitude of cell bodies and their intricately ramifying dendrites, is the same as in the axone. In fact Gasser and Graham have produced evidence to show that it is not. Now conditioning is not a matter of mere conduction across axones. Its neural basis is brain tissue, not only of the cortex, but, as Marquis has shown, the lower central nervous structures as well.

Thus not only are the time factors different, but there is danger in uncritically applying processes which hold for the peripheral nerve to the central nervous system.

Furthermore the objection on the basis of the time factor holds even more emphatically if we attempt to apply the refractory phase theory to Kantrow's phenomena of silent

extinction and the decrement which occurs with continued pairing of the conditioned and unconditioned stimuli.

The next problem is to fit the heterochronic principle to these data. Lapicque discovered that in addition to the intensity factor there is a time factor in the excitability of nerve tissue. It is defined as the minimal time which it takes a stimulus current of arbitrary intensity to excite a nerve. There are different chronaxies for different nerves and different muscles. Impulses will pass from a given nerve to a given muscle if the two have the same chronaxie, or if their chronaxies have certain ratios. In this view isochronism is the condition for the excitation of a response and heterochronism accounts for inhibition. The theory could probably explain Wenger's datum with the supplementation of some factor which will account for a sudden change to heterochronism from a state of isochronism in all three of our experiments, if it were not for the fact that there are some fundamental physiological difficulties.

Chemical theories of inhibition are the most attractive of all, and they are buttressed by a history of excellent experimentation. The first were Ringer's (25) investigations on the perfusion of the heart muscle of the dog by potassium, calcium, and sodium chloride solutions, followed by Howell's (13) work. Sherrington (26) used it to explain reciprocal inhibition in the spinal animal. Recently Hoagland (11) has applied the principle to the explanation of sensory adaptation in cutaneous and muscle sense organs. He suggests that in spite of lack of evidence, it be applied to the central nervous system. The theory is founded upon his experiments on the effect of potassium upon adaptation of sense organs. There is a ratio of  $K$  ions between the inner and outer parts of these tissues—the  $Ki/Ko$  ratio. Normally the ratio is  $10 \cdot Ki/Ko = 10$ . When the  $Ko$  factor is built up on the tissue, inhibition occurs. This view fits in nicely with the fact that in Ringer's solution the potassium salt is inhibitory to heart muscle.

If now this concept is applied to our three data, the lapse in each case is explained as a piling up of  $K$  ions on the surfaces of such brain tissues as are involved in the decrement in

conditioning. The time factor here presents no difficulty but the view needs supplementation to account for the reversal from excitation to inhibition.

The membrane theory of Lillie (17), if rigidly carried to its conclusion, is reduced to a refractory phase theory. Usually it is applied in connection with the chemical theory as is the case with Hoagland.

Among the general theories then there remains to be considered McDougall's drainage hypothesis. Briefly this assumes the presence of a reservoir of free nervous energy in the nervous system. This energy gets directed by stimulation into various parts of the organism. On this view inhibition is the mechanical draining of neurin from one part of the system to another. It reminds one of Descartes' concept of the flow of 'animal spirits' and scarcely needs refutation. If a volume of energy really is drained, it could be verified in ten minutes in an experimental animal. The notion is squarely at variance with the nature of the nerve impulse. If there is a constant quantity of neurin it is difficult to explain why a generalized decrement in conditioning over a long or short period occurs.

So much for the technical interpretations. There are several secondary theories which make no assumptions concerning the biochemical mechanisms of excitation and inhibition. They are concerned rather with the two phenomena as they are found.

One such view is known as peripheral inhibition. It assumes that there are excitatory and inhibitory efferent peripheral fibers which innervate each muscle. Such an arrangement is known to exist in the large pincers of the crayfish and in the powerful adductor muscle of bivalve molluscs. They are definitely present in the autonomic nervous system, but are not present in the parts of the nervous system which controls striate or skeletal muscle. So we may summarily discard this view so far as conditioning is concerned.

Indeed it is precisely the condition of skeletal muscle which is involved in the process of conditioning since the responses of this type of muscle are used as criteria of conditioning. And

this brings us directly to Wenger's proprioceptive facilitation theory. This is also a theory of inhibition. At this point we should re-examine his report. He says, "Although it has been long known that infant responses are quite variable, it seemed unusual that a subject, when no sign of self-stimulation was apparent, should suddenly not respond after having given several successive responses to the conditioned stimulus. These states of no response seemed to occur when the subject was very quiet, and were frequently followed by sleep. Of equal interest was the observation that conditioned responses first seemed to appear when the infant was very slightly active, and also that it was under conditions of very slight activity that the conditioned response re-appeared after having disappeared. The latter fact strongly suggested spontaneous or autonomous disinhibition by tactal or proprioceptive stimulation. Yet it was occurring here in infants not only before conditioning was complete, but before any specific attempt had been made to build up internal inhibition" (31, p. 299).

These observations suggest that so far as body activity is concerned, there is an optimum during which conditioning occurs. The optimum appears to be a situation of not too great general body activity. During quiescence, conditioning fails. Now in terms of the nervous system, this means that tonic impulses arising proprioceptively in the muscles during body activity are pouring into the central nervous system. Such proprioceptive stimulation is the basis of posture and Freeman (7) calls the process in the nervous system the postural substrate. On the basis of this approach Wenger has formulated two following principles of conditioning (31, p. 307). (1) "Decrement in muscular tension usually will be accompanied by a general decrement in response." (2) "Increment in muscular tension usually will be accompanied by a general increment in response." On the basis of these maxims, he makes an application to the process of conditioning by suggesting that an inhibition of the conditioned response is due to reduction of the proprioceptive substrate. In other words, the reduction or the increase of proprioceptive processes

occurring in the central nervous system accounts for inhibition or facilitation of conditioning. It will be seen at once that this formulation is consistent with the experimental evidence in the studies of Leyton and Sherrington, Graham-Brown, Lashley, Travis, and Bishop. There is also the suggestive experiment by Harlow, who paralyzed the striate musculature of the ape and found that without proprioceptive stimulation from this musculature, conditioning failed. Moreover, Wenger's view avoids the *tabula rasa* error, which Pavlov repeated in his doctrine of spatial projection.

There are several qualifying comments to be made about this proprioceptive facilitating and inhibitory theory, one of which should not be overlooked; namely, that the source of the central neural pattern or substrate is not exclusively proprioceptive. The matter is well illustrated by the experimental work upon tonus involved in postural reactions. In these antigravity behavior patterns, innervation originates in a number of sense organs including proprioceptive, vestibular, cutaneous, labyrinthine and visual organs. The neural substrate pattern underlying this behavior pattern involves processes going on in the spinal cord, the cerebellum, Deiter's nucleus in the medulla, the red nucleus in the midbrain, and the cortex of the hemispheres. Thus the activating forces of the nervous system are not merely proprioceptive; they are also extero- and interoceptive. Moreover, and this is something all of us more or less forget, it is intimately and constantly influenced by the fluid matrix of the body, by what Claude Bernard called the *milieu interne*.

Now Wenger's proprioceptive view of inhibition was designed to account for the result of his own experiment. Even if my qualifications are neglected I think his view succeeds in its purpose, but it could not contemplate Kantrow's two subsequent findings. Take the instance of silent extinction. Here in addition to proprioceptive inhibition we probably must have recourse to another factor, namely the fluid matrix. For in a state of satiation there is a temporary shift of blood from the peripheral musculature to the gastrointestinal organs, and the consequent temporary state of

relaxation is accompanied by a reduced bombardment of impulses from muscle sense organs. Quite decidedly the *milieu interne* is a factor to be considered here, but the immediate factor should be sought in the tonus condition of the muscles.

In Kantrow's second datum paired conditioning was continued over a much longer period and for more frequent training units than is usual. This period lasted for several days. After the fifteenth unit the strength of the conditioned response dropped from an initial level to a lower plateau. Now the tonus conditions of the infants as measured by amount of body activity remained at the same average level during both plateaus. The graphs show, however, that during the lower plateau body activity was much more variable than on the initial one. Apparently a steady tonus state is necessary for maintaining strong conditioning, whereas a variable condition of tonus weakens the response. Quite evidently this phenomenon can be considered as an instance of the tonus theory, provided that the optimal tonus condition varies only within rather narrow limits. This notion may be implicit in the concept of an optimum, but Kantrow's findings render it necessary to state it explicitly. Under the conditions of this experiment the factor making for variability cannot be hunger. Some other factor, possibly in the fluid matrix, must be responsible for the tonic variability. Interpreted neurally this may mean that under the influence of this matrix certain interoceptors bombard the central nervous system and compete with both conditioned and unconditioned stimuli. However, this is going beyond any direct evidence.

The formulation of the theoretical problem of infant conditioning as it has been presented here makes no attempt to include all of the data in this field of research. Its extension to them is, of course, desirable. The treatment here is merely an effort to select from an array of possible theories one which may be taken as 'the best fit' to a restricted group of data resulting from our work with infants. It assumes that in the conventional Pavlovian interpretation there are some limitations, and it takes a modest first step toward a different theory of conditioning.

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## THE LAW OF EFFECT OR THE LAW OF QUALITATIVE CONDITIONING

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The relation of Thorndike's 'Law of Effect' to Pavlov's 'Laws of Conditioning' has been in the course of the last thirty years, perhaps, the most discussed fundamental problem in objective and behavioral theories of learning. An excellent recent review was given by Hilgard (6) and an ultra radical dichotomous solution has been offered by Skinner (47). The primary purpose of the present article is, however, not so much to deal with the status of the problem as to suggest a new formulation of it. It is the contention of the writer that the law of effect is a special higher form of conditioning that may best be called qualitative conditioning, in which a quality, a direction, a tendency, an affect—a vector or a valence, if you prefer—is the chief conditioning datum. It will be further contested that, while this qualitative conditioning, unlike typical quantitative or mere-linkage conditioning, operates only within a special group of responses and manifests a number of characteristics that are not predictable or derivable from known facts of quantitative or mere-linkage conditioning, still the two types of modification possess a sufficient amount of common properties to warrant their inclusion under one term. Again, it will be maintained that in ordinary learning situations there is often an actual contradiction between 'effect' and 'mere linkage,' the quality and the quantity<sup>1</sup> of the conditioning or the learning. Finally, it will be shown that, if the effect-producing stimulus is noxious in nature, the contradiction occurs not only between 'effect' and 'linkage' but also between two opposing 'effects': one

<sup>1</sup> Some readers might prefer 'molar' and 'molecular' or 'general' and 'specific' to 'qualitative' and 'quantitative.' The greater suitability of the last two terms will, however, become more apparent as the article progresses.

when the nocuous stimulus is applied and the other when the nocuous stimulus ceases. The last two points have already been discussed in a somewhat different fashion by Schlosberg (45) and by Mowrer (25).

We might begin our argument with an illustration of the two forms of conditioning in their most typical manifestations. Fortunately, we do not have to build up hypothetical cases but may take our examples directly from the laboratory. Consider, first, an experiment from the series of investigations by Konorski and Miller (15-20; diagram of laboratory in Fig. 1). The flexion of the right hind leg of a dog, produced by

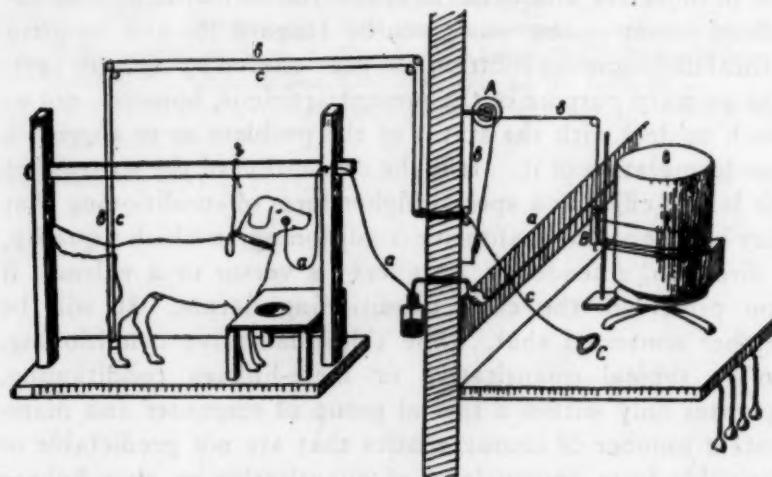


FIG. 1. Diagram of one of the Konorski and Miller set-ups in Pavlov's laboratory. *a*—system to measure the dog's salivation; *b*—system to measure the flexion of the dog's leg; *c*—system to produce the flexion.

the experimenter's exerting a calibrated amount of pull on an attached string, is repeatedly followed by feeding the animal. After a few trials it becomes quite clear, from the much reduced leg resistance, that the flexion response has become greatly facilitated, or that the connection between pull and flexion has been very much strengthened. Control experiments indicate that this response-facilitation or connection-strengthening is by no means a result of repetition alone but is

a direct function of the feeding.<sup>2</sup> Very soon after, it is noted that it is no longer necessary to pull the dog's leg to produce the flexion but that attaching some instrument to the leg or even merely placing the animal in the stand may also be effective. Parallel with the facilitation of the flexion response and its extension to other stimuli, the animal develops also food reactions such as salivation, head turning, and the like to the pull, the flexion, and the stand. When food is repeatedly withheld, the salivation is reduced to zero but the modification of the flexion response is somewhat more complex: the animal begins to extend rather than to flex his leg.

The other example will be taken from one of the many cases of simple conditioning in Pavlov's laboratory. A dog is placed in a stand and a metronome is sounded in its vicinity. As the dog turns towards the metronome, food is presented to him, although strictly speaking the food is tendered irrespective of the dog's response. After a few combinations of the metronome with the food, it is noted that the animal's so-called investigatory reactions to the metronome become weakened and disappear. Control experiments indicate that, while this weakening and disappearance may be produced by repetition alone, it is greatly hastened by the metronome-food combination. Very soon the response to the metronome begins to assume very definite food-seeking and food-consuming characteristics: salivation, chewing, and general bodily orientation towards the source of the food. When food is repeatedly withheld, the acquired food responses of the metronome gradually weaken and finally disappear and the original investigatory responses reappear.

A few more very significant statements must be added to complete the account of the two examples and to illustrate 'effect' and conditioning with noxious or abient stimuli. In

<sup>2</sup> The writer does not share Skinner's (46) view of the complexity of the Konorski and Miller *S-R* situation. Skinner's contention that the situation contains its own reinforcement (relief from tension) and that the food is only a new substituted reinforcement is plainly contradicted by all facts of this type of conditioning. It should be mentioned, however, that Skinner's analysis refers to a somewhat different and older set-up in which the dog's leg was not only flexed but raised a certain distance to touch a horizontal bar. Konorski and Miller's later studies were conducted in Pavlov's well-controlled laboratory.

the first example, if the flexion of the leg is followed by leading 0.25 per cent of hydrochloric acid instead of food into the dog's mouth, the modification is quite different. Upon pulling the string, the leg resistance goes up, and the flexion finally turns into an extension. But note in either case—food or acid, flexion or extension—pulling the string or placing the animal in the stand produces a nearly equal amount of salivation. (If a more definite nocuous stimulus is used, such as a strong blast of air into the animal's ears, the flexion-extension reversal is even more clear-cut.) In the second example, the investigatory reaction to the metronome—the correlate of the flexion response—always decreases, and the decrease is, provided conditioning takes place, independent of the nature of the succeeding stimulus whether it is food, acid, or shock. If the succeeding stimulus is nocuous, the metronome comes to assume nocuous or abient response characteristics, and as a rule simple nocuous or abient *CR*'s differ little from food or adient *CR*'s. However, it should be mentioned that evidence has recently accumulated to show that, on the one hand, nocuous *CR*'s, unlike food or adient *CR*'s, may become more stable by the omission of the nocuous stimulus (12, 45) and that, on the other hand, nocuous 'effects' may actually strengthen preceding *S-R* connections (25-27).

The similarities and the differences between the two modifications, presented here as typical laboratory cases, may now be summed up and re-stated. The first obvious similarity is of course the fact that in either case *all* modifications resulted from contiguity, the activation of two separate *S-R*'s in close succession. The second similarity is that in both cases *functionally new* connections were formed between stimuli of antecedent *S-R*'s and responses of subsequent *S-R*'s—pull → salivation, metronome → salivation, placing in stand → flexion. Third, in both cases repeated withholding of the conditioning stimulus weakened<sup>8</sup> the new connection.

The differences are somewhat more impressive and more cogent. First, the subsequent *S-R*'s in the second case in-

<sup>8</sup>The special case of the strengthening of a new connection through withholding its conditioning nocuous stimulus will be dealt with separately, later in the article.

variably weaken their antecedent  $S-R$ 's, while the subsequent  $S-R$ 's of the first case can, depending upon their nature, strengthen or weaken—or reverse—their antecedent  $S-R$ 's.<sup>4</sup> Second, extinction, or repeated withholding of the stimulus of the subsequent  $S-R$ , restores the original response of the antecedent  $S-R$  in the second case, but reverses the antecedent  $S-R$  in the first case. Third, the modification phenomena of the second case are common characteristics of any successful linkage between  $S-R$ 's, but the specific modifications of the first case are evidently confined to only a special group of subsequent and antecedent  $S-R$ 's. The subsequent  $S-R$ 's of the group are popularly known as rewards and punishments, but their scientific delimitation is far from certain. The boundaries of the antecedent  $S-R$ 's of the group are even less well-defined, although there are indications that they do not include autonomic responses (40, 47). Recapitulating the main features of the two cases, we may state that *while forming a connection between a stimulus of an antecedent  $S-R$  and a response of a subsequent  $S-R$  and weakening an  $S-R$  by a subsequent event are universal attributes of any modification by contiguity, the strengthening of an  $S-R$  by a subsequent event is a manifestation of a special contingency.*

Now, past attempts to relate the modifications of the two cases and general discussions of 'effect *vs.* conditioning' appear to the writer to have suffered, as a rule, from a basic confusion in that analogous parts have not been placed in juxtaposition. Comparisons have been made between—what Lewin might call—phenotypes rather than genotypes: between  $S_{\text{antecedent}} \rightarrow R_{\text{antecedent}}$  of the first case with  $S_{\text{antecedent}} \rightarrow R_{\text{subsequent}}$  of the second case. (In the specific examples cited here it means comparing pull  $\rightarrow$  flexion with metronome  $\rightarrow$  salivation rather than pull  $\rightarrow$  flexion with metronome  $\rightarrow$  investigatory response.) The fact was often neglected that 'effect' does not strengthen a new but an old connection and that the effect's true analogue in conditioning is a postulated but unconfirmed phenomenon, designated by Hull as alpha condi-

<sup>4</sup> The special problem of the 'effect' of noxious stimuli or 'punishments' upon antecedent  $S-R$ 's will be treated separately in the latter part of the article.

tioning, or the "sensitization and augmentation of the original unconditioned reaction to the conditioned stimulus" (9, p. 431). This neglect gave rise to two imaginary difficulties. First, 'back-effects' were not discriminated from 'back-connections' (backward conditioning), and, since 'back-connections' are uncommon or may even be non-existent (backward conditioning may be regarded as a special case of forward conditioning to a trace), it was argued that 'back-effects' are also unlikely. Secondly, it was pointed out that while in conditioning a connection is strengthened by a similar event, *i.e.*, metronome → salivation + food, in 'effect' it is strengthened by a heterogeneous event, *i.e.*, pull → flexion + food (Grindley, 3; Hilgard, 6). It is obvious, however, that when the proper analogues are used, the two alleged discrepancies between 'effect' and 'conditioning' practically disappear. There is apparently no greater retroaction when food strengthens a preceding connection between seeing a lever and pressing it than when food weakens a preceding connection between hearing a metronome and turning towards it. Similarly, there is little difference between the heterogeneity of flexion and food and that of any number of *CR* set-ups, when corresponding analogues are compared.

Again, largely to offset the two hypothetical discrepancies between 'effect' and 'conditioning,' a common contemporary account holds that the strengthening of a modifiable connection between a stimulus and a response by a subsequent reward is only indirectly a function of the reward itself and that its main strengthening comes from anticipatory or reward-preceding *C-R*'s (4, 10). This allegation, too, is not well borne out by careful experimental analysis. In the experiments of Konorski and Miller, in which the action of preceding *C-R*'s could be isolated, it was found that these *CR*'s had actually interfered with the strengthening of the main connection of pull → flexion by the food. While it is possible that eventually this interference would be converted into a facilitation, still the results strongly suggest that, at least in the initial stages of training, the chief energy source of the strengthening of the connection between pull and flexion

emanates directly from the reward itself, presumed retroaction and heterogeneity notwithstanding. To repeat, the problem of 'effect,' or what Skinner has called '*R*-conditioning,' is not a problem of true conditioning in itself but of alpha conditioning, one of the phases of true conditioning. In typical *CR* experiments 'effect' or alpha conditioning does not as a rule manifest itself, but, on the contrary, the antecedent response, if anything, tends to weaken and disappear. The specific issue of the problem is, then, what are the conditions, the factors, and the mechanisms that cause or are correlated with this 'effect,' or alpha conditioning, in a *CR* situation. For one thing, this correlation appears to be with some complexity of organismic behavior (*cf.* 45), since no 'effect' or alpha conditioning was found in autonomic responses (40) or in simple reflexes (5). For another thing, 'effect' seems to involve reinforcing responses that definitely direct or orientate the organism. The writer's concepts of quantitative and qualitative conditioning are in place here.

In the writer's view, conditioning may be quantitative and qualitative. The terms 'quantitative' and 'qualitative' are used here with reference to the scope and degree of organization of the total organismic behavior involved in the conditioning act. Quantitative conditioning denotes the conditioning of a specific, segmental, component response, a conditioning that is to a large extent self-contained and independent of integrated organismic orientation. Qualitative conditioning signifies the conditioned interaction of organismic qualities—sets, tendencies, directions, affects, vectors—an interaction that is primarily polar and holistic rather than gradational and atomistic. Quantitative conditioning is genetically older, is more basic or universal; qualitative conditioning is superimposed or emergent but is more lasting and effective. Qualitative conditioning contains quantitative conditioning as a subsistent and the contradictory actions of the two kinds of conditioning throw light upon a number of seemingly inconsistent results in ordinary learning and conditioning. The naming of the organismic qualities that govern *CR* situations is a difficult task. Adient and abient, 'what to do' and 'what

not to do,' 'go to it' and 'get away from it' are perhaps the most suitable general names. Other names may be: approach-avoidance, escape-stay, resistance-submission, rejection-acceptance, like-dislike, right-wrong. A few concrete illustrations will help to make clear the rather abstract expositions of this paragraph.

In an experiment by Warner (53) the subjects were rats unrestrained in their activities, the conditioning stimulus was an electric shock, and the conditioned stimulus was a buzzer or a light. Warner concludes that the new behavior aroused by the conditioned stimulus produced only the same end-results as the conditioning stimulus but that the neuromuscular mechanisms employed in the two situations differed. "The four rats which quite consistently scrambled under the fence in response to the shock did learn to get to the other side of the fence in response to the sound—but by *leaping over it*" (p. 113, author's italics). The behavior conditioned in Warner's experiment was evidently a general escape tendency and not a specific movement or response. In the series of investigations by Ivanov-Smolensky and his numerous collaborators (13, 14) young children were conditioned to press a rubber bulb at the presentation of some signal in order to obtain food. The *CR's* were often, however, just general manual adient reactions: the child was conditioned to 'do something' when a light was flashed or a buzzer sounded, but the 'specific doing' may well have been, at least in some stages of training, more a function of the situation with which the child was confronted than of the situation to which he was conditioned. Liddell, James, and Anderson (22) state that the shock *CR* of their sheep "is not stereotyped but is, on the contrary, notably plastic. . . . The pattern which the conditioned reflex exhibits depends upon the circumstances under which it is elicited . . . the 'conditioned' animal seeks to defend itself in the manner *appropriate to the situation*" (p. 54-55, authors' italics). In Wickens' study adult human subjects, who were conditioned to make an extensor movement of their middle finger, just as readily made a flexor movement, when their hand was turned over to a palm-up position. Again, subjects who

had their flexor *CR*'s extinguished were found also to have lost their extensor *CR*'s (56). A general shock-avoidance tendency was conditioned rather than a specific response. In the experiment of Miller and Cole (24) a voluntary manual adient reaction greatly facilitated the establishing of an eyelid *CR*, while a voluntary manual abient reaction—resisting the doing of something—even more spectacularly hastened the extinction of the lid *CR*. In a study by the writer (34), in which human subjects were conditioned to salivate while solving a maze, the set or feeling of right or wrong produced wide variations in the salivary conditioning. Some hitherto unpublished results of the writer that bear more specifically on the problem of qualitative *vs.* quantitative conditioning will be given in greater detail.

Short musical compositions, colored slides of paintings, and life-size photographs of young college girls were presented individually to 30 subjects under 3 different conditions: (a) when the subjects were eating, (b) when the subjects were hungry waiting to eat with food right next to them, and (c) right after their meal. The purpose of the experiment was, as might be suspected, to study the changes in salivation and in affective attitudes to the different stimuli-items under the three different experimental conditions. The experiment was conducted between 12:15 and 2:00 o'clock in the afternoon and the subjects were totally ignorant of its purpose. It was found, however, that the conditioned changes of salivation and of affectivity differed considerably from each other. True, in both cases the greatest modifications were produced for stimuli that had been presented during the eating periods, and high positive correlations were obtained between the affective and the salivary changes in these periods. But, conditioned salivations were also very substantial for items applied during the hunger-periods when affective modifications were reverse or negative, and no conditioned salivation was manifest in the after-meal periods, even though the changes in affectivity were then high and positive. In general, conditioned salivation and conditioned affectivity were separate and independent in the sense that each could occur without the other.

But, when the two occurred together, affectivity changes significantly influenced the course of the salivary conditioning. Furthermore, this influence was reliably greater when the affectivity changes were positive—increases in affectivity—than when they were negative—decreases in affectivity—which is interestingly in line with the findings of Hilgard (7) on voluntary supplementation of eye-lid conditioning and with Thorndike's re-statement of the 'Law of Effect.' Finally, the results gave some indication that the conditioned affectivity was more of a nature of alpha conditioning than that of the usual substitute beta conditioning: the affectivity for the esthetic objects—musical compositions, paintings, and photographs—did not just assume the food affectivity but was merely strengthened or weakened by it, retaining its own specific quality.

Thus, experimental evidence on quantitative and qualitative conditioning seems to justify the following five statements. (1) An organismic quality, or a general behavioral or experiential orientation, may be the chief conditioning datum in a *CR* situation. (2) An organismic quantity,<sup>5</sup> or a specific response or movement, may be conditioned with little or no influence from general behavioral or experiential orientation. (3) In ordinary *CR* or learning situations the two conditionings interact with each other. (4) In this interaction the qualitative conditioning is primarily the directive factor and its direction may be positive or negative, adient or abient. (5) Qualitative or central conditioning is more of an alpha than of a beta type. There is a strengthening or weakening of one quality by another rather than a substituting of one for the other.

We are now ready to apply the principles of quantitative and qualitative conditioning to the problem of effect *vs.* conditioning. The diagrams in Fig. 2 will be of aid and Skinner's experiments will be taken as specific examples. A

<sup>5</sup>The writer must repeat that he uses here the terms 'quantity' and 'quality' in only a relative manner. An organismic quantity—a specific movement or response—has of course also its qualities, and an organismic quality—a tendency, attitude, or affect—can, to be sure, be quantified.

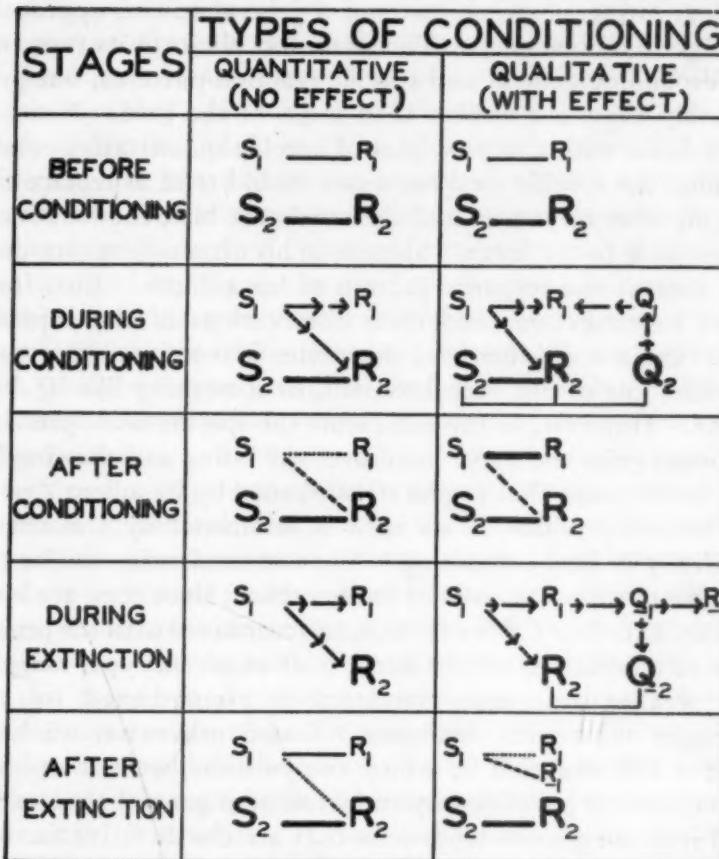


FIG. 2. Schema of  $S-R$  changes in adjacent  $CR$  situations with and without 'effect.' The thicknesses of the lines represent the strengths of the  $S-R$  bonds. Broken lines are conditioned bonds and dots are weakened or extinguished bonds. Arrows increasing in size indicate a strengthening process and arrows decreasing in size a weakening process.  $Q$ 's denote central qualities or tendencies, and negative subscripts a reversal of a response or a tendency.

rat presses a lever in response to some stimulus <sup>6</sup> and the lever pressing is followed by food. The food arouses in the animal two kinds of responses: specific food movements such as

<sup>6</sup> Skinner's concepts of operants, correlations with stimuli, and correlations with responses are quite baffling to the writer who sees no need for their postulation. Their alleged common sense and operationism are outweighed by the contorted assumption that they force upon 'effect' phenomena in cases in which the stimulus is quite definite and overt.

seizing, biting, chewing, etc. and a general adient, approaching, 'go to it' tendency. The lever, too, elicits in its turn two corresponding general and specific reaction-patterns, but presumably these are weaker than those of the food. Now, in accordance with the principles of specific quantitative conditioning, the specific food responses should tend to replace the specific lever responses, and the rat should bite, chew, salivate in response to the lever. Skinner in his ultra objectivity does not present the response pattern of his animals. But, from other investigations and from observations of the animals' behavior in a Skinner box, the writer is convinced that to a considerable extent such behavior, or something like it, does occur. However, in the end-result the specific lever-pressing response gains the upper hand over the biting and chewing for the mere reason that it gets strengthened by its adient, 'go to it' tendency which in its turn is reinforced by the adient tendency of food-consuming. The two tendencies, unlike the specific movements, are not in opposition, since they are both adient, and their *CR* interaction, in accordance with the principles of qualitative conditioning, is of an alpha type, namely, the weaker antecedent tendency is strengthened by the stronger subsequent tendency. Stated otherwise, we have here a *CR* situation in which competition between specific movements is paralleled by cooperation of general tendencies, and it is the general tendencies that are the directive factors. (No doubt some specific movements are also common to the lever and the food situation which would facilitate the cooperative task of the general tendencies.)

The question, why do not the original responses to the conditioned stimuli in typical Pavlovian set-ups become strengthened as a result of general adient tendencies, may best be answered by examining the nature of these responses. These responses are, first, investigatory, the overt manifestation of which is a not very consistent 'turning to' reaction, and second, they are responses the general tendencies or qualities of which are abient and thus antagonistic to the adient reinforcing tendency of the food. The weakening of the responses of the second class is naturally easy to account

for, since their general tendencies are themselves weakened rather than strengthened by the subsequent adient tendencies of the food. In the experiment of Erofeeva (2) in which a shock to a dog's hind leg was followed by food and in the study of Slutskaya (48) in which infants were pricked by a pin and then fed, the withdrawal responses consequently disappeared and were eventually replaced by food reactions. In the successful cases of Slutskaya's investigation the infants first stopped crying at the sight of the pin (apparently the weakening of the general abient tendency), then ceased withdrawing from it (weakening of the specific abient response), and finally attempted to seize the pin and made swallowing movements at its presentation (transformation of abient into adient response). However, the failure of food to strengthen preceding investigatory responses cannot be readily explained in terms of general antagonistic tendencies and its reasons apparently lie elsewhere. At present the best assumption seems to be that these investigatory responses are merely too insignificant organismically to arouse any central tendency or quality of their own which could interact and be reinforced by the central food-consuming tendency. This organismic insignificance may well be due not only to the insufficient gross bodily action and displacement of these responses but also to their inadequate emphasis and inconsistency of occurrence in the total *CR* situation. Grindley has shown, for instance, that, if a particular movement is isolated from the general investigatory response-complex and is invariably reinforced by the food, the movement does become strengthened as a result of the feeding (3). Still, the statement that any connection between any *S-R* is strengthened by subsequent reward of food is far from justified. Some qualitative organismic orientation towards the antecedent *S-R* appears to be a prerequisite.<sup>7</sup>

The special case of 'effect *vs.* conditioning' with subsequent punishment or absence presents two main problems. First,

<sup>7</sup> Preliminary results by the writer indicate that, if human subjects are totally unaware of the antecedent *S-R*, no 'strengthening by reward' is effected. However, if the subjects are aware of the total antecedent situation, strengthening extends even to parts of which the subjects are not specifically aware (40).

according to conditioning, an antecedent connection between a stimulus and a response should become weakened by a subsequent noxious or abient event inasmuch as the antecedent stimulus becomes conditioned to the subsequent abient response. In accordance with the 'law of effect,' however, no such weakening should occur, recent experiments even indicating that a connection may actually become improved by a subsequent shock (25-27). Secondly, there is the question of avoidable *vs.* unavoidable shock and of free animals *vs.* restrained animals. This aspect has been excellently analyzed by Schlosberg (47). The chief argument here is that while in restrained animals the strengths of withdrawal *CR*'s are to a large extent a function of the number of reinforcements by the shock, in free animals such *CR*'s are better fixated when after initial training the shock is omitted whenever the animal makes the proper avoidance reaction. A corollary of this argument is that even in restrained animals conditioning is more 'regular' for diffuse and vegetative responses that have little 'success' relation to the organism than for precise withdrawal and defense reactions in which the factor of 'success' does enter.

The answer to both problems hinges upon a neglected analysis of the special role of abient stimuli, specifically shock, in learning or conditioning. In two earlier reviews (29, p. 99, 101, 104; 31, 114) the writer has pointed out that, compared with food *CR*'s, shock conditioning is much more variable, less readily stabilized, more resistant to extinction, and more subject to 'reinforcement-decrement'—decrease rather than increase of *CR* after reinforcement. Upon a more thorough mathematical treatment and an inclusion of a greater number of experiments, the special characteristics of shock conditioning turned out to be much more radical in nature and much more complex and multiple in origin. It appeared quite definitely that the 'best fit' for curves of shock conditioning and of extinction of shock conditioning requires the postulation of reverse or negative factors that interfere respectively with the regular course of the conditioning or the extinction. The writer assumes that the negative factor in the conditioning

results from an adient, relief from pain or discomfort, tendency which is aroused centrally in the organism when the shock or nocuous stimulus ceases. The divergence between the extinction of food and shock *CR*'s is even more complicated and may be better understood, if we reflect somewhat subjectively on just what happens to an organism when food or shock is omitted in a *CR* situation. In the first case an animal commences a set of food reactions—chewing, swallowing, salivating, etc.—in response to, let us say, a buzzer. But, since the food is not there, the animal misspends his activities and his deficit reactions necessarily produce, what for lack of a better term may be called frustration, or a reversal of his adient into an abient tendency. However, when shock is omitted and an animal free to move about runs away, let us say, from a buzzer as if it were a shock, there is no such misspending of activities, no special unfulfilled deficit reaction, and consequently no frustration and tendency reversal—at least not before the occurrence of the next reaction. The two cases of extinction are quite distinct. Being conditioned to 'get something' and the 'something' *does not occur* is different from being conditioned to 'get away from something' and the 'something' *has not occurred*.

Thus, we are probably justified in saying that the inconsistencies and variances of 'effects' and 'conditionings' of nocuous stimuli are due to the dual role which these stimuli play in such situations. They are paralleled centrally not by one but by two opposing tendencies: abient when the stimuli are applied and adient when the stimuli cease. To this may be added the fact that intense nocuous stimuli may disrupt organismic behavior to the extent of masking typical learning and *CR* modifications and that, on the other hand, mild nocuous stimuli may be readily transformed from conditioning reinforcing agents into conditioned signalling stimuli. Again, we may contend that the faster extinction of shock *CR*'s in restrained animals is to be attributed to the greater discomfort that these animals endure upon making avoidance reactions. Then, it may be further maintained that the reason for the *CR* advantage of non-reinforcement by shock in free

animals is that, when the shock is omitted, the interfering adient, relief from pain, tendency is not well pronounced, as presumably relief from anticipated pain cannot be as adient in character as relief from actual pain. Finally, it may be argued that the greater shock *CR* regularity of diffuse and vegetative responses is to be ascribed to the fact that these reactions are not well, if at all, represented centrally by qualitative directive tendencies.<sup>8</sup>

#### SUMMARY

1. Experimental evidence indicates that as a result of contiguity the chief conditioning datum may be either an organismic quantity, a specific movement or response the exercise of which is virtually independent of total bodily adjustment, or an organismic quality, a general behavioral or experiential orientation the quantities or specific movements and responses of which may be quite variable.
2. Qualitative conditioning is primarily of an alpha type: the strengthening or the weakening of one quality by another; quantitative conditioning is mostly of a beta type: the substitution of one response for another.
3. Qualitative conditioning may greatly influence, modify, or even reverse the course of quantitative or specific conditioning. This influence is much more pronounced when the qualitative conditioning is positive or adient than when it is negative or abient.
4. Past discussions of the relation of the 'law of effect' to true conditioning have obscured the problem by not comparing analogous parts of the two situations. The chief difference between the two may well be accounted for by assuming that in an 'effect' situation there is conditioned not only a specific response but also a general organismic quality, tendency, or affect.

<sup>8</sup> The diagrams of Fig. 2 and the discussion at the beginning of the article show also extinction differences between 'effect' and 'no effect' *CR* situations with subsequent adient stimuli. These differences are, however, not very basic, since *CR* reversals upon extinction are not unknown in typical Pavlovian set-ups (see 39 for the conditions of their occurrence). Still, these reversals are practically the rule in 'effect' conditioning and probably account for Skinner's failure to obtain 'disinhibition,' as this phenomenon does not manifest itself when the *CR*'s are reversed.

5. Evidence indicates that a nocuous stimulus arouses in the organism two contradictory central tendencies: an abient when the stimulus is applied and an adient when the stimulus has just ceased. The conflicting *CR* interaction of the two opposing tendencies within the same stimuli offers an explanation for the variable and seemingly inconsistent effects of nocuous stimuli in learning and conditioning.

6. When the adient conditioning stimulus in a *CR* situation is omitted, the organism's acquired adient tendency towards the conditioned stimulus is quickly reversed into an abient tendency. No such ready reversal occurs when an abient conditioning stimulus is omitted in a *CR* situation. Stated otherwise, while false reward-signals readily assume the characteristics of punishments, false punishment-signals do not as a rule become rewards.

7. The evidence brought forth in this article supplements the writer's results on pattern conditioning inasmuch as both suggest dynamic levels and functional organization of conditioning. Pattern conditioning deals with levels of stimulation, qualitative conditioning with levels of responding.

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## SYMBOLIC TECHNIQUE IN PSYCHOLOGICAL THEORY

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Techniques for obtaining extremely accurate data have been developed in many fields of psychology, but the theoretical tools with which these data are manipulated have not received commensurate development. Clumsy theoretical treatment of accurate results has often rendered insignificant the numbers in their decimal places, and made of no avail the experimental care taken to make these values accurate. Much of this clumsiness lies in the dull edge of the vernacular, which even with a large addendum of scientific terminology is often not a fit tool for theoretical colloquy.

Data in some fields of psychology—for example sensation and learning—have reached an accuracy which, if they are to be significant, justifies more careful theoretical treatment than unselfconscious prose can give them. In other fields of the science—for example personology—this stage has not been reached, and it is quite probable that such care would not be justified. However, workers in these vaguer realms must always keep in mind the great uncertainty and large probable error ever present in their work.

Faults of the vernacular have frequently been emphasized: its common ambiguity, even in the hands of careful writers; its elliptical nature; the illusory character of its abstract words; the difficulty of discovering a referent for many words; the slipperiness of easy-flowing phrases; the increasing difficulty of understanding precisely what a word means as it becomes more commonly used; the connotations which ride with words, forcing us unwillingly to read between lines; the possibility of ‘punning,’ with a word meaning one thing in the first half of a sentence and the opposite in the second half; the fact that we can speak figuratively and not realize it, or

that realizing it we can condone it as 'giving scope to the expression.'

Mathematics, mathematical logic, and symbolic logic provide careful techniques for critical theoretical considerations, but few of them have been applied to psychology. Particularly valuable and readily applicable is the symbolic logic which has developed rapidly since Whitehead and Russell's *Principia Mathematica*.

Possibly logical symbolic formulations are not necessarily clearer than very careful prose; possibly they have only heuristic value. Probably, however, there is more justification for them. The care that they entail means continual criticism. They are shorter than prose that is not elliptical. Their meaning does not change readily. They do not easily admit of emotional connotations or added meanings. Such a tool theoretical psychology can well use. Although the technique may be difficult to employ at first, it may for certain purposes become as much of an improvement over prose as were Arabic numerals over Roman. It would be tedious to work logarithms in Roman numerals!

C. L. Hull has done much to bring careful theoretical techniques into psychology. Particularly this has been true in his applications of logical procedures to two 'small theoretical systems' which he has published (3, 4). He has demonstrated at least four advantages which such systems can have (4, 4-9): 1. Clarity and lack of ambiguity; 2. Ability to settle disagreements by careful deductive technique, once the definitions and primitive statements are agreed to by opponents; 3. Productiveness in testing hypotheses indirectly; 4. Productiveness in suggesting topics for future investigation. (As a matter of record, the history of science indicates that important discoveries have only infrequently resulted from such careful procedures. More frequently they have occurred under unexpected circumstances, almost coincidentally. The future must demonstrate whether or not the unproductiveness of logical procedures has been due to the infrequency of their application.)

Hull has not employed the recent advances in logical

techniques, particularly the symbolic techniques, but has retained a rather rough analogy to Euclidean procedure. Nor has he taken the care in constructing his Euclidean system that is found in those of Veblen or Pieri. D. K. Adams is right in saying that he could have been more precise in "finding out how to leave the fewest possible and most immediately evident notions undefined" (1, 214), which is an aim of all deductive systems. There is, moreover, another and more serious reason for Hull's lack of success in achieving the clarity he so rightly desires: he has fallen into some of the errors which accompany almost all use of ordinary language. He has suffered from the elusive way words evade their original meanings and assume others—almost, it would seem, under their own power and independent of their user. Thus it happens that assumptions, of which Hull was probably not aware and which he was no doubt trying to avoid, have entered into his systems.

More than once in recent years behavioristic or mechanistic treatments of theoretical problems similar to Hull's have bogged down in ambiguity. This is not to suggest that only behaviorists and mechanists are illogical. The essence of the behavioristic movement has been to stress empirical clarity, and the writings of behaviorists are perhaps more clear-minded than those of any other group, partly because other groups do not hold clarity at such a high premium. But even the most lucid behaviorists have employed a prose style, whose assumptions in a single page are often so involved that it would be difficult or entirely impossible to convince either the author or any reader that non-behavioristic or mutually incompatible assumptions were basic to the argument. It may well be that much of the apparent success of such objective systems in explaining complex phenomena lies in unrecognized assumptions which the authors have unwittingly, and thus with entire sincerity, allowed to enter.

Hull's systems have been worked out with such care that they can be used to illustrate how these assumptions slip in. Here is a great advantage of such careful procedures as Hull has followed. Criticism of them can be more explicit than is

most criticism. Though errors stand in bolder relief, it is well for science, because they can be the more easily discovered. And if the system, either in an original or in a revised form, can finally withstand careful scrutiny, then it is much more likely to be convincing than is loose hortatory or expository language. Every wise man has at some time had sad experience with the trickiness of writing, with the way in which an apparently flawless argument may leave him unconvinced, admitting, as the farmer did to the lightning-rod salesman, "It sounds all right, but I don't like the looks of it."

Deductive systems are constructed in the following four stages: (1) primitive notions; (2) definitions of terms derived from these; (3) primitive statements in these terms; and (4) theorems derived from these statements. There must be agreement about the meaning of the primitive notions and of any definitions which may be derived from them before further progress can be made. Such agreement may be reached by any sort of convention, but science with its empirical reference has recently tended to achieve it by ostensive or operational definitions. Hull himself has tried to establish in his second miniature system, which we shall consider in detail in this article, "specific or 'operational' definitions of the critical terms employed" (4, 5).

If the primitive notions and the definitions derived from them cannot be understood and accepted by all, it is impossible to pass to stage three, to begin an argument. In a deductive system agreement must precede any disagreement. Nothing meaningful can be said about hippogriffs if no one knows what hippogriffs are. It is true that theorems may be derivable from primitive statements which are not subject to direct proof, but they must be in terms which can be understood. Otherwise there would be no way of telling whether the theorems they generate are correct, because they would be meaningless. Hull says rightly (4, 7) that there are two kinds of statements: those which are directly demonstrable and those which are not. But this does not mean that either sort of statement may be couched in words whose meanings are uncertain. Science insists that all its terms be defined ostensively or be derived logically from terms so defined.

In the presentation of his second miniature system, Hull leaves the status of his postulates uncertain. By *postulate* he seems in some places to mean primitive notion, and in other places, primitive statement. It may be that he has tried to construct his postulates so as to telescope stage one and stage three into a single stage, but it seems more probable that he has entirely neglected to indicate his primitive notions. There are two reasons for believing that his postulates are not simple explanations of the basic notions on which his system is built: (1) because his postulates follow and do not precede his definitions; and (2) because his postulates are couched in terms defined in the definitions. Rather his postulates are primitive statements concerning the existence and method of operation of such phenomena as the stimulus trace and positive association (4, 16). The most important technical error in Hull's system is that the terms used in these postulates are not carefully defined, and their meaning is difficult to determine.

Hull is right in saying that a postulate need not be observable to have a place in a deductive system. If the theorems it generates are provable and significant, it has a pragmatic value. But he is wrong in saying that such postulates can receive indirect verification (4, 7). This is because two incorrect postulates in a system may give rise to a correct conclusion, which is directly verifiable. This situation can be illustrated as follows (the logical operators are defined on page 471;  $p$  and  $q$  symbolize propositions):

$$\begin{array}{c} \sim p \supset q \cdot p \supset \sim q \text{ (false)} \\ \sim p \text{ (false)} \end{array}$$

$\therefore q$  (Which is found to be true.)

Perhaps the truth was:

$$\begin{array}{c} p \supset q \\ p \\ \hline \therefore q \end{array}$$

Hull is wrong (4, 7-9) in asserting that in science the usual

deductive method can be virtually reversed and an argument made from observed conclusions to a certain proof of a single set of premises. This is the fallacy of affirming the consequent:

$$\begin{array}{c} p \supset q \\ q \\ \hline \therefore p \end{array}$$

Let us summarize this point: (1) each term used in a system must have meaning, directly operational or derived logically from operationally defined terms; (2) but any sort of primitive statement may be made in these terms, and need not be directly verifiable.

What is operational definition, which Hull wishes to apply in his system? P. W. Bridgman, father of operationism, says of definition, in explaining the operationistic doctrine of it: "Concepts can be defined only in the range of actual experiment, and are undefined and meaningless in regions as yet untouched by experiment" (2, p. 7).

S. S. Stevens, who has applied operationism in psychology, states the nature of an operational definition by saying that "A term or proposition has meaning (denotes something) if, and only if, the criteria of its applicability or truth consist of concrete operations which can be performed" (6, 517-518).

H. A. Murray has created a new word, *actone*, and has made a careful distinction between it and *action* (5, pp. 55-56). To him an actone is an action pattern *qua* action pattern, and is distinguished from the common use of the word *action* to describe both the act and the effect of the act. He shows that similar actones—putting food in the mouth and putting poison in the mouth—may have different effects and that different actones—pulling a trigger and taking poison—may have the same effect. Essentially operational definition must be in terms of actones rather than actions.

A. N. Whitehead was fond of saying to his Harvard classes, from his long experience as a logician, that, if there is error in

a book, it is in the first two pages where the definitions and assumptions are made. Once these assumptions have been accepted, an author can prove any point, especially if he takes two volumes to do it. Inevitably a deductive system rests on the validity of its definitions. Let us turn, therefore, to Hull's second miniature system (4, 15-16) to see if his definitions are really operational, whether they are either stated in terms of actones or derived logically from primitive notions thus defined.

In making this critique we shall use symbolic logic. The formulations are ready for use in a deductive system, but they have not been so employed in this paper. Rather they are included here for their argumentative and short-hand value, to illustrate how psychological systems can be symbolized, and to indicate their advantages and also their disadvantages, for example, the difficulty of comprehending them at first sight.

The selection of symbols to use in formalizing Hull's system entails a choice of primitive notions by the symbolizer, for Hull has not indicated what his primitive notions are. In doing this we must make certain assumptions, which might well be questioned by philosophers or theoretical scientists. This is necessary because psychology is an advanced science, and must build upon the more basic disciplines. We shall, for instance, take the notion of discrete and discernable temporal points as primitive. If the physicists' conception of time alters, this primitive notion may need to be changed, but the symbolic deductive system makes such a change easy because the effect of any assumption on the system is immediately obvious. If psychologists are to get to psychological questions at all, they must make assumptions whose corroboration or disproof is left to others.

We shall make the following primitive assumptions, which are not of universal applicability, but refer to specific experimental conditions:

Assumption	Primitive Notion	Symbol
1. Individual objects or organisms can be distinguished from each other and recognized.	Object or organism (with spacial relations, as 'between')	Capital letters (except <i>S</i> )
2. Observable parts of objects or organisms can be distinguished from each other and recognized.	Part of an object or organism	Small letters (except <i>s</i> and <i>i</i> )
3. Observable physical stimuli can be distinguished from one another, and like stimuli can be recognized.	Physical stimulus and like physical stimulus	$S_1 \dots S_n$
4. Different times can be distinguished.	A temporal series	$t_1 \dots t_n$ (written subscript)
5. When an object or organism or a part of an object or organism performs an operation in relation to another, the movement can be distinguished and like operations can be recognized.	Performs an operation in relation to (To illustrate the reader may substitute some specific act, as 'approaches' or 'is adient to.')	$\alpha$

Besides these symbols, a number in a circle is used to represent (and state the existence of) what Hull defines in the definition to which he has given that particular number. (Thus in the paragraph below the first symbolic definition, 'A reinforcing state of affairs ①' means 'That a reinforcing state of affairs exists.') Also the real number series ( $1 \dots n$ ) and the following logical operators are employed:

Symbol	Meaning
$\cdot$	<i>and</i>
$\sim$	<i>not</i>
$\vee$	<i>or</i>
$\therefore$	<i>therefore</i>
$\therefore$	<i>implies by definition</i> (may be read : if . . . then)
( ) or [ ] or { }	to mark functional units, as in algebra

We can now consider the first of the definitions of terms critical to Hull's system, in order to find just what part of the definition is 'operational':

"1. *A reinforcing state of affairs* (Postulate 3) is one which acts to give to the stimulus-trace component (Postulate 1) of preceding or following temporal coincidences consisting of a stimulus trace and a reaction, the capacity to evoke the reaction in question (Postulate 2)."

We must assume that the references to the postulates are inserted merely to show where the terms immediately preceding the references are employed, for, of course, such primitive statements or postulates cannot logically enter into the definition of the terms in which these postulates are to be stated. We can symbolize a reinterpretation of this definition as follows:

$$\textcircled{1} \quad \mathfrak{S}^f (A \propto B)_{ts_1} \cdot (g \propto h)_{t_n} \cdot (A \propto B)_{ts_2}$$

That is to say: A reinforcing state of affairs  $\textcircled{1}$  implies by definition that a certain object or organism  $A$  acts in relation to another object or organism  $B$  at the time of a certain stimulus (1), and that one part ( $g$ ) of the organism  $A$  acts in relation to another part ( $h$ ) at a certain time, and that the certain object or organism  $A$  acts in relation to the other object or organism  $B$  at a time later than the time of the first stimulus, when there is another stimulus (2) like the first.

In this reworking of the definition we are assuming what is probably a questionable statement in order to put Hull's system in as favorable a light as possible: that a stimulus trace is operationally demonstrable, as in the neural discharge in the retina of the eel after stimulation has stopped. The stimulus trace operationally, then, is the action of two parts of the organism in relation to each other at some unspecified time.

We can also demonstrate the reaction to the stimulus, and this we have symbolized in the most general way:  $(A \propto B)_{ts_1}$ . We are then informed that the stimulus-trace component has the 'capacity to evoke' a second reaction. Of course the 'capacity to evoke' is not operationally demonstrable; only the mere fact of the second reaction is. There is no available operational technique for showing that the second reaction

came because of the first, and there might well be disagreement that it did. Grounds for such disagreement must not be found in the basis of a careful system. All we can therefore do is to add to the first reaction a second,  $(A \propto B)_{ts_2}$ , occurring when a like stimulus occurs. Thus we arrive at the operational meaning of the first definition, *reinforcing state of affairs*, which is later equated with *goal* (4, 17). This meaning in terms of actones is that a goal or reinforcing state of affairs exists when a stimulus twice elicits the same response, and when at some time there is an action between parts of the responding organism, which certainly would appear to Hull to be an exceedingly poor definition of what is at least commonly meant by a goal or even by a reinforcing state of affairs. Thus it appears that something unoperational has crept into the definition, and it seems certain to the present writer that this is because of the inherent difficulty of dealing with language, for there can be no doubt of Hull's desire to remain objective.

Now let us go on to other definitions:

"2. *Experimental extinction* is the weakening of a conditioned excitatory tendency resulting from frustration or the failure of reinforcement (Postulate 4)."

According to our system this would be symbolized as follows:

$$\textcircled{3} \xrightarrow{d'} (A \propto B)_{ts_1} \cdot (A \propto B)_{ts_2} \cdot [\textcircled{3} \vee \sim \textcircled{1}]_{ts_3} \cdot (A \sim \propto B)_{ts_4}$$

In the longer non-elliptical vernacular this would be: Experimental extinction  $\textcircled{2}$  implies by definition that a certain object or organism *A* acts in relation to another object or organism *B* at the time of a certain stimulus (1), and that this is repeated at the time of a like stimulus (2), and that frustration as defined in  $\textcircled{3}$  or the negative of a reinforcing state of affairs as defined in  $\textcircled{1}$  exists at a third time, and that the certain object or organism *A* does not act in relation to the other object or organism *B* at the time of another stimulus (4) like the first.

In Hull's phrasing of this definition he refers to "the weak-

ening of a conditioned excitatory tendency." This 'tendency' is certainly not operationally demonstrable, and the 'weakening' could be demonstrated only by the fact that at some later time the object or organism does not respond to a stimulus as it did at first. The first two clauses of the definition as rewritten therefore indicate that conditioning exists (*i.e.*, *A* acts in relation to *B* at the times of two like stimuli), and the fourth indicates that at a later time it does not. Hull also says that the "weakening . . . results from frustration or the failure of reinforcement." *Frustration* is defined in his third definition, and the "failure of reinforcement" to which he refers is apparently the negative of ①. We have therefore indicated that at some unspecified time either ③ or the negative of ① exists. Of course Hull should have defined *frustration* before using the term.

It appears that this definition of 'experimental extinction' is not adequate or satisfactory when it is given in terms of actones, operationally. Extinction has usually meant more than simply that, when a certain sort of stimulus is given repeatedly an object or organism acts repeatedly in a certain way, and that, when it is given later, it does not act in that way, and that at some time either frustration as defined in ③ exists or a reinforcing state of affairs as defined in ① does not exist.

For want of space we cannot consider each definition in such detail. Instead there is given below the symbolic form (based on rigid operational definitions) of the others of Hull's first ten definitions, and following it a non-elliptical translation of these symbols into the vernacular.

"3. *Frustration* is said to occur when the situation is such that the reaction customarily evoked by a stimulus complex cannot take place (Postulate 4)."

$$\textcircled{3} \stackrel{df}{=} (A \propto B)_{ts_1} \cdot (A \sim \propto B)_{ts_1} \vee (A \propto C)_{ts_1}$$

Frustration ③ implies by definition that an object or organism *A* acts in relation to another object or organism *B* at the time of a certain stimulus (1), and that the certain

object or organism  $A$  does not act in relation to the other object or organism  $B$  at the time of another stimulus (2) like the first, or acts in relation to another object or organism  $C$  at the later time, when there is another stimulus (2) like the first. (In terms of action and effect, non-operationally,  $C$  might be a 'barrier,' but operationally in terms of actones it is merely an object or organism.)

"4. *Seeking* is that behavior of organisms in trial-and-error situations which, upon frustration, is characterized by varied alternative acts all operative under the influence of a common drive ( $S_D$ )."  
(Hull suggests as an illustration of a common drive, hunger (4, 17), and we shall assume that operationally he means certain contractions of the stomach. We shall therefore let  $s$  and  $u$  represent two parts of the stomach which can be observed by some fluoroscopic or other technique to be moving in relation to each other.)

$$\textcircled{4} \xrightarrow{df} \textcircled{3}_{t_1} \cdot (A \alpha D \cdots N)_{t_2} s_1 \cdot (s \alpha u)_{t_3 \cdots t_4} \textcircled{6}_{t_5 \cdots t_6} s_2$$

Seeking  $\textcircled{4}$  implies by definition that frustration as defined above exists at a certain time (1) and that the object or organism  $A$  acts in relation to a series of objects or organisms  $D \cdots N$  at a later time when a stimulus (3) occurs like the stimuli at the time of frustration, and that the part of the stomach  $s$  acts in relation to the part of the stomach  $u$  at the times referred to, and that a simple trial-and-error situation as defined below ( $\textcircled{6}$ ) exists at the times referred to. (In a technically correct deductive system  $\textcircled{6}$  would have been defined before  $\textcircled{4}$ .)

"5. An *attempt* is a segment of behavior the termination of which is marked by either reinforcement or extinction."

$$\textcircled{5} \xrightarrow{df} (A \alpha B \cdots N)_{t_1} \cdot [\textcircled{1} \vee \textcircled{2}]_{t_2}$$

An attempt  $\textcircled{5}$  implies by definition that an object or organism  $A$  acts in relation to a series of objects or organisms  $B$  to  $N$  at a certain time (1), and that a reinforcing state of affairs as defined above or an experimental extinction as defined above occurs at a later time (2).

"6. A *simple trial-and-error situation* is one which presents to an organism a stimulus complex which tends to give rise to multiple reaction tendencies which are mutually incompatible, one or more of them being susceptible to reinforcement and one or more of them not being so susceptible."

$$\textcircled{6} \stackrel{df}{\supset} S_1 \dots S_n \cdot A \alpha B \dots N_{tS_1 \dots S_n} \cdot \textcircled{1} \cdot A \alpha K \dots M$$

A simple trial-and-error situation  $\textcircled{6}$  implies by definition that a number of stimuli ( $S_1$  to  $S_n$ ) exist, and that the object or organism  $A$  acts in relation to a series of objects or organisms  $B$  to  $N$  at the time of these stimuli ( $S_1 \dots S_n$ ), and that a reinforcing state of affairs as defined in  $\textcircled{1}$  exists at the time that the object or organism  $A$  acts in relation to the objects or organisms  $K$  to  $M$ . (In saying that "one or more of them [are] not . . . so susceptible" Hull does not necessarily imply that extinction exists, so the symbol for *extinction* is not included.)

"7. A *correct* or 'right' reaction is a behavior sequence which results in reinforcement."

$$\textcircled{7} \stackrel{df}{\supset} \textcircled{1}$$

A correct or 'right' reaction  $\textcircled{7}$  implies by definition that a reinforcing state of affairs as defined above exists.

"8. An *incorrect* or 'wrong' reaction is a behavior sequence which results in experimental extinction."

$$\textcircled{8} \stackrel{df}{\supset} \textcircled{2}$$

An incorrect or 'wrong' reaction  $\textcircled{8}$  implies by definition that experimental extinction as defined above exists.

"9. *Discouragement* is the diminution in the power of one excitatory tendency to evoke its normal reaction, this diminution resulting from one or more unsuccessful attempts involving a second reaction."

$$\textcircled{9} \stackrel{df}{\supset} (A \alpha B)_{tS_1} \cdot (A \alpha C)_{tS_2 \vee tS_3 \dots n}$$

Discouragement  $\textcircled{9}$  implies by definition that the object or organism  $A$  acts in relation to the object or organism  $B$  at the

time of a certain stimulus (1), and that the object or organism  $A$  acts in relation to the object or organism  $C$  either at the time of a later like stimulus (2) or at the times of a series of later like stimuli (2...n).

"10. A behavior sequence is said to be *directed* to the attainment of a particular state of affairs when there appears throughout the sequence a characteristic component ( $r_a$ ) of the action ( $R_a$ ) closely associated with the state of affairs in question and this component action ( $r_a$ ) as a stimulus tends to evoke an action sequence leading to the total reaction ( $R_a$ ) of which the component constitutes a part."

Let  $Q$  be an object or organism between the objects or organisms  $A$  and  $B$ . Then:

$$\textcircled{10} \stackrel{df}{\supset} (A \propto Q)_{t_1 \vee t_2 \dots n} \cdot (A \propto B)_{t_{n+1}}$$

Direction (of a behavior sequence)  $\textcircled{10}$  implies by definition that an object or organism  $A$  acts in relation to an object or organism  $Q$  (between the objects or organisms  $A$  and  $B$ ) at a certain time (1) or a certain series of times (1...n), and that the certain object or organism  $A$  acts in relation to the certain object or organism  $B$  at a time ( $n+1$ ) later than the first mentioned time (1) or the last of the series of times (1...n).

Since the unselfconscious language of the proofs of the theorems in Hull's system and their complicated nature would force a careful analysis of them, unwarranted space would be consumed. At best the theorems, even if faultlessly proved, would in all probability be a series of insignificant statements if the words in them were actually defined operationally as Hull wishes. For example, his Theorem II states that:

"Both correct (right) and incorrect (wrong) reactions may be set up by the conditioning (associative) process."

Translated into symbols this would read:

If  $\beta$  represent the adaptive behavior which Hull is referring to as 'the conditioning (associative) process,' and which he postulates (Postulate 2), then:

$$\begin{aligned} \beta \cdot & \{(A \propto B)_{t_1 s_1} \cdot (g \propto h)_{t_n} \cdot (A \propto B)_{t_1 s_2}\} \\ & \vee \{(A \propto B)_{t_1 s_1} \cdot (A \propto B)_{t_1 s_2} \cdot [\textcircled{3} \vee \sim \textcircled{1}]_{t_1} \cdot (A \sim \propto B)_{t_1 s_4}\} \end{aligned}$$

which is the long form of:

$$\beta \cdot (7) \vee (8)$$

$\beta$  is postulated and is not verifiable, and the rest of the statement is not particularly significant in explaining the phenomena of adaptive behavior, which was the purpose of the system.

Hull's intentions are right—to use logical method to clarify theoretical issues and strengthen theoretical positions, thus showing how they follow directly from operations. It will be greatly to the advantage of the science if such procedure is followed where it is applicable.

He is wrong, however, in giving the false impression that his arguments are logically coherent because of the quasi-classical format in which they are cast. Many of his logical procedures are technically incorrect. Many undefined notions slip in all through his work. If he wishes to achieve precision, let him subject his theories to the purging discipline of symbolic or non-elliptical forms. He may then well find that strict behavioristic primitives explain adaptive behavior much less satisfactorily than he has maintained. The most serious fault that can arise from the use of such a quasi-classical system as Hull's is that it may give the impression that the complexity of human adaptive behavior can be adequately explained in terms of such simple mechanical notions as he uses. In concluding his article he asks (4, 30):

"But what of consciousness, of awareness, of experience—those phenomena of which the philosophers and theologians have made so much and upon the priority of which they are so insistent? An inspection of the postulates of the miniature system of adaptive behavior presented above certainly shows no trace of any such phenomena. It is clear, therefore, that so far as that considerable array of complex behavior is concerned, consciousness or experience has no logical priority." He does not use the word *consciousness*, but he does use *capacity* and *tendency*—vague, undefined notions which might mean *consciousness* or *hippogriff* as far as the reader is concerned. It is not clear whether consciousness is or is not

logically prior to the behavior he describes in his second system. Nothing has been proved by the system which has any reference to consciousness, because it is not certain what, if anything, is primitive.

A symbolic regimen might have made it certain.

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## THE EFFECT OF OUTCOME ON LEARNING

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In a recent series of three articles in the *Journal of Psychology*,<sup>1</sup> T. L. McCulloch takes issue with my own attempt to state the manner in which the effects of action influence learning. An examination of his argument shows it to be very well founded and the general statement of the way in which learning progresses through the removal of drive which is contained in *The Psychology of Human Conflict* requires some revision.

This general statement as quoted by McCulloch was as follows:

"Excitement brings increased activity and this brings stimuli in new orders. Responses tend to become more varied. . . . Each response as it occurs is associated with the drive but loses this association to the next response. . . . Eventually a response (consummatory response) removes the drive. For this in turn the drive becomes an associative cue. . . . (p. 102). The next time that the disturbers are present they will tend to call out, by virtue of their last association, the act that removed them. Other acts associated with them have been dissociated or unconditioned by the next act. But after successful removal of the disturber, *it is no longer there to be associated with a new act*. The drive remains faithful to the act that removed it because that was its last association. After that no new associations could be established because the drive is gone" (p. 98).<sup>2</sup>

To this general statement McCulloch has a number of valid objections. One of these is that a continuing internal drive instead of remaining faithful to the response that removed it would have its association with that response

<sup>1</sup> *J. Psychol.*, 1939, 7, 293-316.

<sup>2</sup> *The Psychology of Human Conflict*. New York & London: Harpers, 1938.

destroyed on the next occasion by the initial behavior of search or pursuit.

This is, of course, quite evident. To serve as an associative cue for a specific action a stimulus must have specific characters, a specific pattern reserved for the associated response. Not the physiological drive of hunger in the form of continuous or recurrent stomach spasms but some pattern of action to which this has led must be the cue for any detail of hungry behavior.

Another objection, and one that admits of no argument, is that in many learning situations an animal is given repeated trials and rewarded after each 'correct' run with a bit of food that obviously does not remove hunger. Bruce's study would even indicate that a slight feeding at the beginning of a maze improves learning and presumably increases motivation.

In place of my suggestion that the successful act or series of acts is learned because it is always the last association with the drive and that this association remains because the drive has been removed by the consummatory response, McCulloch suggests that the effect of the correct or successful action is not to remove the drive but to inhibit the precurrent restless behavior and to reduce excitement. This he illustrates from his observations of clasping in the young chimpanzee. Clasping puts an end to distress and reduces excitement.

This observation is well founded. In very many instances actions are obviously established as responses to a troubling situation though they do not remove the drive in the same sense that they remove the stimulation originally responsible for a state of excitement or distress. I had suggested in *The Psychology of Human Conflict* that the habit-forming nature of thumb-sucking was owing to the fact that that act inhibits crying and other restless movements and thereby causes excitement to diminish. The effects of morphine were explained in terms of its relaxing and quieting effect, and not in terms of the removal of the annoying situation that had produced distress. In fact, the source of activity and restless excitement is often complicated or obscure and is not properly described in terms of drive. The word "drive"

should be used for specific stimuli or organic states that have specific forms of relief. Hunger and sex can be so described, but not the anxiety of a young chimpanzee that follows being alone in a strange place.

When I attempted in *The Psychology of Human Conflict* to formulate a general description of the habit-forming nature of success I fell into the old language of drive and consummatory response which Sherrington had made familiar. The essential mistake of that generalization was to speak of the removal of an annoyer instead of speaking of the removal of the annoyance. The latter can be made to disappear through inhibition by competing activities while the annoyer remains. Morphine is habit-forming because it reduces muscular tension and excitement; it does not remove the injury that gave rise to pain or the spouse with whom the subject had quarreled. In the addict it is probably true that it does remove the 'abstinence effect' that is responsible for recurring distress, but it is the cure of the distress and not the removal of the cause that accounts for the habit-forming quality.

The rat at the end of the maze may be given only a small pellet of food, not enough to put an end to the state of hunger which made him restless, but enough to cause the rat to stop and eat. The precurrent activity, the running about and exploration which have been in evidence up to that time are ended by the discovery of food. McCulloch would probably grant that there is in many cases a decided difference between the case in which an exciting drive like hunger is actually removed and the case in which restlessness is temporarily relieved. Thumb-sucking stops crying and reduces excitement. It becomes associated and remains associated with the particular pattern of excitement that preceded it, but relief occurs only so long as the presence of the thumb in the mouth continues to inhibit restlessness. The thumb can not long compete with the bottle or the breast. These are even more habit-forming because they remove the maintaining stimuli for excitement.

In the case of the cats in a puzzle-box, a problem upon

which Horton and I have been working, the source of excitement is not readily described as a drive. It is not absolutely necessary for the cats to be hungry in order that they shall become restless in the box and that they shall eventually discover and fix the action that releases them. Occasionally a cat will lie down in the box unless it is hungry. But if the cat is restless from any cause and will only move about and explore the box, it will eventually trip the photo-electric or mechanical release and escape and in that case it promptly leaves by the open door. The important fact is that in leaving the box it leaves behind the situation to which it had made this last response. Even after a long interval this situation retains this association with the successful act. There has been no chance for unlearning this association because the cat has not in the meantime been in the box.

This does not, of course, mean that on the next occasion the instant the cat is placed in the box it will respond with its act of escape. The box-situation is not simple. We have observed that on this next occasion when the cat happens to be in the same part of the box it tends strongly to repeat its former behavior in series except for what may be regarded as accidental short-cuttings of the action. When it enters the box it tends, unless distracted by something new, to take the path that it had on the last occasion. When it arrives at a place where it had formerly risen on its hind legs, jumped for the wire screen at the top, clawed at a corner, turned left or right, there is a high probability that this will be repeated. We use the term association because it is the former association that enables us to predict the present action.

In many instances long series of movements are repeated, but these are subject to external and possibly internal interruptions. A fly moves across the field of vision and the direction in which the cat walks is changed. For a time behavior leaves the groove and does not resume it until the cat finds itself in a familiar stance in a familiar corner.

McCulloch has only one slight misconception of my own position. At no time have I suggested that drive-removal

has the status of a principle of learning. All that was intended is that when the drive-removal occurs (and this should now be amended to indicate removal of the annoyance rather than removal of the annoyer) the result is to remove the cues for the successful action. This protects them from re-association. In the puzzle-box these cues for the escape include those offered by the box itself and those deriving from the cat's movements about the box. *The learning took place on the first performance before escape or reward. All that escape or reward does is to protect the learning from being unlearned.*

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## A NOTE ON KELLOGG'S TREATMENT OF SKILLS

BY JAMES M. LYNCH

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That the need for unity in the field of educational psychology is keenly appreciated is shown by the many recent attempts at syntheses of the various theories of learning. Some authors<sup>1</sup> point to the common ground in each of the different explanations, and assert that a description and interpretation of all learning phenomena *in general* is a possibility. Others hold that the different explanations apply to different types of learning: conditioning, learning of skills, perceptual learning, and conceptual learning.

A third way of bringing the theories together, Kellogg's<sup>2</sup> eclectic view, sees no point in trying to reduce all learning to a single general principle. Learning is classified under two headings: 'Learning by Addition' of responses (conditioning) and 'Learning by Subtraction' of responses (trial-and-error). The four learning theories discussed by Kellogg are applicable, it is held, to these two classifications. Furthermore, a distinction is made between original learning and the learning of skills. None of the theories of learning discussed applies to the acquisition of skills.

It is with this separation of skill-learning from all other kinds of learning that this note is concerned.

Kellogg proposes a sort of scale or continuum, of which trial-and-error learning and Gestalt sudden-insight learning are the poles. Conditioning and sign-Gestalt learning, too, are viewed as varieties of a common process. Moreover, there is no hard and fast boundary between these two sets of learning theories. "It is a boundary which can easily be

<sup>1</sup> J. F. Dashiell. A survey and synthesis of learning theories. *Psychol. Bull.*, 32, No. 4, pp. 261-275.

<sup>2</sup> W. N. Kellogg. An eclectic view of some theories of learning. *PSYCHOL. REV.*, 1938, 45, 165-184.

bridged. Difficult or elaborate problems, in actual practice, may be said to include learning which is describable in terms of any, or of all, of the theories.”<sup>3</sup>

In his treatment of skills, however, this “no hard and fast boundary” procedure disappears completely. Skills are placed sharply apart from and beyond the types of learning accounted for by the learning theories. The development of a skill, it is maintained, can be explained as the learning which goes on after the original learning—after the right reactions have been integrated.

The perfection of a skill has nothing to do either with ‘addition’ or ‘subtraction’ [of responses] in learning. It is not concerned with the beginning of a new response or series of new responses, or with the elimination of old responses. Rather is it concerned with the giving of a certain fluency, speed and precision to complete reactions after their nature has already been determined. Improvement in skill is not a matter of finding out what is to be done; but of doing it better and better. Skills may, therefore, be placed under the heading of overlearning. Their single most important principle is the Law of Exercise, upon which their efficiency must ultimately depend. They start from the point where the [trial-and-error, insight, conditioning, and sign-Gestalt] theories already considered end and they move forward from that point.<sup>4</sup>

This complete separation of skill learning from original learning, it seems, is out of harmony not only with Kellogg’s view, which bridges the distinction between ‘low’ or mechanical learning and ‘high’ or ideational learning, but also with the theories he discusses.

From the standpoint of the conditioning theory, for example, improvement in skill in writing, skating, and playing the piano, *as well as the original learning* of these things, are affairs in which new  $S \rightarrow R$  combinations are formed. The motions of the arm alone in a skilled activity, such as the

<sup>3</sup> *Ibid.*, p. 182.

<sup>4</sup> *Ibid.*, p. 183.

hitting of a baseball, involve a great many muscular contractions and expansions, the proprioceptive stimulation of each of which gives rise to subsequent responses. In the pattern  $S$  (baseball)  $\rightarrow R$  (hitting with bat), the  $R$  itself consists of pushing the bat back slowly, pulling it forward rapidly, and guiding it so as not to miss the ball. Furthermore, within each of these movements, pushing back, pulling forward, and guiding, there are innumerable minor  $s \rightarrow r$ 's occurring in conjunction with one another, that is to say, there are numerous  $s \rightarrow r$  groups which become new combinations or component conditioned responses. During practice (exercise), the conditioning of the smaller  $r$ 's to kinesthetic  $s$ 's results in a progressive variation of response and greater muscular coordination; many of the actions that were made at first are eliminated, just as  $R_1, R_2, \dots, R_7$  in Kellogg's illustration were eliminated, and the whole performance with the bat develops into a smooth, efficient technique. Thus, the perfection of skill *does* involve new responses and the elimination of old responses.

Similarly, in the insight theory, both learning and improvement in learning are explained as matters of finding out what is to be done. Both the acquisition of any form of behavior and the increase in its fluency and precision are matters of discovery and of the creation of new response patterns.

Consider, for example, a child in the second grade. He has learned to write legibly, but the large, shaky-looking letters he makes reveal the presence of many little push-and-pull movements within the larger movements that shape the letters. Later on, he comes to make smaller and smoother letters, and finally, he reaches the stage of a free and easy cursive style. According to the Gestalt theory, as set forth by Koffka, Köhler, Wheeler, and others, these writing movements are at each stage—the crude, the fair, and the perfected—an entirely new group of responses. Just as the original learning to write was a new response-pattern which emerged from generalized movements of the arm-as-a-whole, so the

learning to write better and better is a unique response-pattern which emerged when the learner *found out* how to balance his flexor muscles against his extensors.

Hence, since the acquisition of complex skills can be adequately accounted for in terms of any, or of all, of the learning theories, in the same way as maze, problem-box, and puzzle learning, there is no point in regarding skills as beyond and apart from these sorts of learnings. On the basis of the theories themselves and on the basis of Kellogg's own view of the theories, consistency demands that continua be imagined to exist between the development of skill and the descriptions of the original learning.

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## ON THE NATURE OF SKILLS—A REPLY TO MR. LYNCH

BY W. N. KELLOGG

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I owe Mr. Lynch my thanks for his well-stated amendment or correction to the eclectic view of learning theories which I have proposed. My original article grew out of efforts to clarify my thinking on the nature of learning.<sup>1</sup> It gave my own versions—or perversions—of four of the better-known theories of learning, and pointed to certain similarities and differences between them. It mentioned skills, but only very briefly. In fact, it said much less about them than Mr. Lynch has said in his note.

After reading this note I find myself in general agreement with almost everything it contains. I would be quite willing to do what Mr. Lynch has done with skills, and might have done so in the first place, had I thought of it. There are only three minor reservations which prevent my accepting the Lynch amendment completely.

1. It seems to me that Lynch's statement of my position is—how shall I put it?—a little harsh. The quotations which he gives have, of necessity, been taken out of their long-winded context and so don't seem (as he has written them) to carry quite the same meaning I intended them to carry. This, of course, is the inevitable result of abstracting a few statements from a longer background. I said this, for example, which Mr. Lynch did not quote:

"There has been up to the present no mention in this paper of the acquisition of complex skills such as mastery in typing, in speaking a foreign language, or in playing tennis. Is it necessary to invent an entirely new hypothesis to account for this sort of development? We think not. The develop-

<sup>1</sup> W. N. Kellogg. An eclectic view of some theories of learning. *PSYCHOL. REV.*, 1938, 45, 165-184.

ment of a skill can be explained as the learning which goes on after or beyond the sorts of learning which have already been discussed." <sup>2</sup>

Now these words were not meant to indicate that skills should be placed "sharply apart from" (to quote Mr. Lynch) the types of learning which were examined earlier in the article. They probably meant, in my thinking at the time, that after I had finished putting the four theories together, I had skills left over and didn't know what to do with them. If one wishes to think of all learning, from the beginning to the end of it, as a sort of continuum, I could certainly find no objection to this. In fact, I subscribe heartily to the idea.

2. But skill to me *does* represent an advanced stage of learning. No doubt I am defining the word in terms of my own concept of it. Yet somehow it seems to me improper or inaccurate to describe such a thing as the rat's gradual elimination of blind alleys in maze-running as 'the development of a skill.' The animal does not become truly skillful in the maze until *after* he has learned the correct pathway. *Once he has passed that point*, if practice is continued, he may begin to cut corners, increase his rate of speed in covering ground, and so become more efficient in running the pathway which he has previously mastered. This is the sort of thing I would call skill. Skill is learning which is near the maximum of efficiency. It is learning which is close to what used to be called the 'physiological limit' when that term was in vogue. Only the tail end of the process deserves, in my estimation, to be dignified by the name skill. Skill emphasizes the ultimate refinements, as against the crude inaccuracies which are present in the early stages. But, after all, it is only a matter of emphasis.

3. The greatest point of difference which I see between my concept of skill and that of Mr. Lynch, has to do with the nature of the responses that are considered. I have thought of a 'response' as something rather gross and crude, something which might include a lot of separate muscular contractions

<sup>2</sup> *Ibid.*, p. 183.

and expansions, and similarly a lot of neural and other physiological changes in the organism. Thus a 'response' might be 'going into a blind alley' or 'turning to the right.' Again, in the case of conditioning, the 'response' is usually an extremely complex thing. The flexion reaction of a dog, for example, involving as it does the crossed-reflex, together with a shift in the animal's balance, may take in half the muscles of the body.

Mr. Lynch, on the other hand, seems to have analyzed his 'minor' responses right down to what may be called (I put words in his mouth) 'single discrete muscular contractions or expansions of whatever size.' The specific muscular contractions must certainly change in the development of skillful behavior. There can be no doubt of that. At the same time the gross reactions involved in a learned act (as, for example, the straight runs and the turns which the rat himself makes as he goes through the true pathway of a maze) cannot change very much. The general pattern of the behavior need not change, even though the elements in it are reduced and modified.

And so perhaps the difference between us has arisen because of different meanings of the word *response*. I, for one, am not sure how it ought to be defined. Maybe Mr. Lynch is right.

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